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A MEDIUM OF COMMUNICATION
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ROYAL ASTRONOMICAL SOCIETY.

Session 1879—80.

The December meeting was held at the Society's rooms on Friday, the 12th December, 1879.

Lord Lindsay, M.P., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

Mr. Glaisher announced that 58 presents had been received since the last meeting, including several volumes of the "Annales de l'Ecole Polytechnique," which were missing from the series in the library of the Society.

The President read a letter from Mr. Lee, enclosing a letter from Mr. Bartlett, Churchwarden of Stone, requesting the Society to subscribe out of its funds towards the restoration of the Parish Church, which it was suggested had fallen into bad repair while the Society were patrons of the living.

The letter went on to explain that it was proposed that the sum of £3,000 should be expended on the restoration of the church, and the casting of new bells. The President stated that the Council had had the letter under their consideration, and had come to the conclusion that they could not subscribe out of the funds of the Society to such an object, but that an announcement should be made in order that Fellows of the Society who felt so disposed might contribute.

Mr. Stone showed to the meeting a new form of reading micrometer which he had devised, and which he described as a compromise between the German and English forms.

Mr. Neison was called on to read a paper by Professor Newcomb *On the error in mean longitude of Hansen's tables,* in which the author pointed out that the variations in the mean error of longitude, to which attention had been lately drawn by Mr. Lynn and others, could be accounted for by the omission from Hansen's tables of the term of 17 years period, which had been shown by Mr. Neison to be due to the action of the planet Jupiter on the moon. This term, which varies as the sum of twice the angular distance between Jupiter and the lunar perigee, reached its minimum influence in the years 1860 and 1877, and its last maximum in 1868, its next maximum being 1885. The omission of this term would therefore produce variation in the apparent error of mean longitude of the tables similar to those which have been lately made known. In the paper this imperfection in the theory is taken into account, and the probable error in the moon's mean longitude predicted for the next few years. At present the observations seem to indicate an error which is nearly 2″ too large, but this Professor Newcomb believes to be due to the effect of the personal equations of the Greenwich and Washington observers, those observatories being the only two where regular observations of the moon are now made.

Mr. Neison then read a paper by himself *On the correction for personality required by the observations of the moon made with the Greenwich transit circle,* a paper dealing with one of the main points raised in the previous paper by Professor Newcomb. On investigating the correction required by the theory embodied in Hansen's lunar tables, by comparing them with the observations made at Greenwich, satisfactory results were obtained when the error of the moon's mean longitude were deduced separateiy for each year, showing that the theory of the terms of long period had been rendered accurate. When, however, the errors of mean longitude for each year were compared together, with the view of investigating the correction required by the theory of the terms of long period, several sudden changes in the error of mean longitude were discovered, which were too sudden to be due to imperfections in the theory, and which seemed to be due to causes within the observatory itself. This led, therefore, to an examination of the personal equation of the different observers, with the view of ascertaining whether the changes which had occurred in the observers would account for these sudden changes in the error of mean longitude. It had hitherto been assumed by investigators that where, as at Greenwich, the observations were equally divided amongst several observers, the mean of all the observations would be sensibly free from the effect of their personal equation. Therefore, as in the course of time the observers were gradually replaced by others, it was assumed that no sensible systematic error would be introduced into the mean error of the moon. Between 1870 and 1874, three observers, Messrs. Dunkin, Ellis and Carpenter, were replaced by three others, Messrs. Lynn, Downing, and Thackeray. But whereas the three former observers had an average large negative personal equation the three latter observers who replaced them had an average still larger positive personal equation. Consequently, whilst the correction for personality during the earlier period—1863 to 1869—remained constant, between 1870 and 1875 it increased by sudden jumps more than a second of arc. To bring, therefore, the observations between 1863 and 1879 to a uniform standard, so that they could be properly compared with one another, important corrections for personality must be applied to the mean of each year's observations. The paper supplied the values of these corrections for each year between 1863 and 1876.

Mr. Dunkin: At what date did Mr. Neison commence these comparisons?

Mr. Neison: 1863.

Mr. Dunkin: Because, of course, it would be more important and valuable if he had gone further back and compared the personal equations of former observers. For instance, if he had gone back to 1847, when the altazimuth was first erected, or better still to 1836, then we should have seen distinctly the course of errors right away from the observers with the old instruments, such as Messrs. Henry, Ellis senior, and Rogerson; because, after all, Hansen's tables are founded on observations made at Greenwich not in recent years, but in past years, and I should like to know whether the errors apply equally to the old observers as to the modern observers. But in regard to differences of personality, we all know there are tremendous personal equations in observations of the moon. After the altazimuth was erected it was remarkable to see every day the up and down motion of the alternate errors, as given by two observers, but it was found that one of them agreed exactly with the older observers, especially with Mr. Henry. I should, therefore, like to know the difference, if Mr. Neison has found it, between the old and new observers.

Mr. Neison: Of course as I do not know which, if any, observer had really observed the true error of the place of the moon, I had to assume some standard to which to refer the observation. For this purpose I assumed that the mean of the observations of the first five observers would be free from error. This is an arbitrary selection, but then it is theoretically immaterial what standard you fix on. Any change would merely vary the apparent error of each observer, but leave quite unaltered the relative amounts, and these are alone of importance. As regards the older observers their personal errors could only be derived from a comparison with Burckhardt's tables, but these are not sufficiently accurate for good results to be obtained. The assumed errors of Hansen's tables, before 1863, are obtained by the assumption that the difference between Burckhardt's tables and Hansen's tables would be the same at midnight as at the time of the moon's transit at Greenwich. This, however, cannot be assumed with any accuracy. I regret that Mr. Dunkin and Mr. Ellis have ceased to observe the moon, as it would be of great advantage if we could get observations of theirs during the next two or three years.

Mr. Dunkin: I mentioned at the last meeting that 10 years ago I introduced a paper to the Society on personality in observing the limbs of the moon, which of course referred only to the first four observers named by Mr. Neison—Dunkin, Ellis, Criswick, and Carpenter—and the results given in my paper agree very much with those given by Mr. Neison. Therefore I can believe in Mr. Neison's deductions, and very remarkable they are, but they are not new, because we have always known that great personalities in observing the limb of the moon will occur.*

Mr. Ranyard read a paper by the Rev. T. W. Webb, *On a new gaseous nebula in Cygnus*, which was discovered on Nov. 14th with Mr. Webb's 9·3 silver-on-glass reflector, by With, while observing with a power of 50. It looked like a bluish, slightly nebulous, star of the 9th magnitude. Observed with powers of 212, 355 and 450 it was seen as a nebulous disk surrounded by a feeble glow about 4 seconds in diameter. At first he thought it might be a comet, but Dr. Copeland, of Lord Lindsay's observatory, had since identified the object as 4004 of Argelander's zone +41. According to the description of Dr. Copeland, it is not round, and has a sharp nucleus near the north preceding edge, and when examined with the spectroscope, shows three lines corresponding very nearly with the ordinary gaseous lines of nebulæ. Owing to its small size, these lines are very easily seen with a McClean spectroscope or an ordinary prism spectroscope.

The President said that his attention was called to this nebulæ by Mr. Ranyard, who had received information of it from Mr. Webb. He had examined it, and it certainly was a very remarkable object. He had given notice of it to Dr. Winnecke, of

---

* [Note by Mr. Neison.] The object of my paper was to show the important variation in the correction to the mean error in longitude which must be applied to reduce each year's observations to the same standard. The existence of personal equations had been clearly pointed out by Mr. Dunkin 10 years previously.

Strasburg, who had examined it with the orbit-sweeper of the Strasburg Observatory, and reported that it looked like a star of the 8th magnitude seen out of focus when the object-glass is improperly centred. It was oblong in the direction of position angle 136°, the greatest diameter being 5·5″, and the least 4·9″; the spectrum of the nucleus appeared to be continuous.

Mr. Knott also read a paper on the same subject. He had examined its spectrum, and he found it to consist of one bright line of considerable intensity. He was surprised to see it so easily with such a small aperture as 7½-in. With a high power it appeared like a bluish star of the 8.5 magnitude with a bright bluish-white nebulosity.

Mr. Ranyard: Is there any evidence of change of magnitude? since it was catalogued as a star by Argelander.

Mr. Knott: I think not. I should say there was no change.

Mr. Knott then read a note relating to a new red star in Canis Minor, which had been discovered by Mr. Baxendell.

The President said that Mr. Baxendell sent him a message with respect to this new star, and its place had been determined pretty accurately at Dun Echt, as in right ascension 7h. 34m. 45·64s. and declination 8h. 39m. 39·6s. In the spectroscope the star showed no peculiarity, and no particular colour seemed to predominate in the spectrum.

Mr. Sadler: Are we to understand that this red star is not in Mr. Birmingham's catalogue?

The President: I do not know whether it is in the catalogue. It is supposed to be a new star. The message sent by Mr. Baxendell ran, as far as I can recollect—"Discovered last night new star in Canis Minor."

Mr. Common was called upon to read a paper *On observations of the satellites of Mars.* He said: I do not think the paper will be interesting to read. It will read better in print, unless, perhaps, the part relating to the way in which the observations were taken. Deimos was seen on Sept. 21st and 25th, Oct. the 15th and 21st, and Nov. the 2nd, 5th, 11th and 15th, and Phobos on Nov. 2nd, 5th, 11th, 13th and 15th. It is a matter of surprise to me that no one else has seen the satellites of Mars this opposition, because I found Deimos equal in brightness on Nov. the 2nd (that is about opposition) to Enceladus, and Phobos was a little brighter still, that is, taking into consideration the glare of the planet and estimating the magnitudes as if the glare were absent. About the character of the light, they seemed to me quite different to the satellites of Saturn. This may be due to the absence of any apparent disks which one fancies one sees in the satellites of Saturn. On the 2nd of Nov. the moon was very bright, but it had no apparent ill-effect on the visibility of the satellites, and if anything it was rather of an advantage. I was very careful, owing to the wish expressed by the Rev. T. W. Webb, to look out for a third satellite, and I observed a great many small stars near Mars, but they all moved in relation to the planet, or, rather, the planet moved in relation to them, but there was no motion as of a satellite. As to the way in which the observations were taken, Mr. Christie advised the use of a dark glass, covering half the field of a negative eye-piece. I had before been using as a cover for the planet in observing the satellite, a steel disk, in fact, a watch hand, put into the micrometer; but Mr. Christie's suggestion induced me to do away with the steel disk and substitute a very small piece of dark glass. I found it a most useful thing while observing the satellites of Mars. The way in which I did it is described in the paper.

Mr. Common: My other paper is a *Note on Mimas and Hyperion.* I only saw Mimas with the 37-inch telescope; a few months ago I had strong doubts whether it existed at all. (Laughter.) I have been trying to find it for about two years with my 18-inch; the curious thing is that I had letters from an observer who was using about half the aperture, and he gave me the times when he saw it at its elongation, exactly at the time Mr. Marth gives it in his ephemeris; but I looked regularly with a friend and I never saw it. However, I happened to be observing Saturn with the 37-inch on Sept. the 25th, when I expected Mimas to be going off, I saw it coming up to elongation. At 9.15 I found it to be approaching the ring; at 10.50 it appeared to be very much closer, and conjunction was estimated to take place at 11.40. I saw it again on many occasions, and I have no hesitation in saying that the ephemeris of Mr. Marth is from 1½ to 2 hours out.

I have, also, some observations of Hyperion. On Nov. 11th it was seen west, and on Nov. 13th and 15th it was seen approaching superior conjunction. On the 18th just after superior conjunction. From a very careful orbit I have laid out, I consider it very likely to be Hyperion.

Mr. Marth: I shall be extremely glad at last to get observations that may be relied upon. If I have been misled it is by observations made by several observers. There may be some mistake, and I should like to know what the error really is. My endeavour has been to represent the observations of Herschel in 1790 and observations made in 1864 at Malta. I know that those epochs are trustworthy. At present I do not see how these observations of Mr. Common can be reconciled with anything like ordinary regular motion. What we want is regular observations for some years to come, and not only at elongation but when the satellite comes up to and passes the extremity of the ring. I have begged (a voice, "and prayed") for such observations for years past, and I hope Mr. Common will be able to furnish a few more, and then it will be possible to make next year's predictions more trustworthy than those I have given for this year.

The President: With regard to Mr. Marth's last observation, I hope the weather will be favourable for Mr. Common to prosecute his observations.

Mr. Marth: I hope we shall have it decided whether the satellites of Mars can be seen after Dec. the 16th, because on that depends whether they will be seen at the opposition of the planet two years hence or not.

Mr. Sadler: I saw what, I think, there is very little doubt, was Mimas, through Mr. Common's smaller telescope on the 31st August, 1877, and it was considerably under the 13th magnitude of Struve's scale. I do not think, therefore, that it would be visible with anything under a 15-inch aperture.

Mr. Neison: After what Mr. Sadler has said what I have to say will sound strange. In 1878 I was observing the satellites of Saturn with Mr. Campbell's 6½-inch refractor, and we both independently saw what we considered to be a satellite, and made out to be Mimas. Mr. Campbell picked it up first, and marked it down independently, and then we compared our observations afterwards, and came to the conclusion that it was Mimas. The next day I brought the observations up to London, and on meeting Mr. Marth in the library I asked him to tell me if I had seen Mimas, and he said it was very doubtful, as the observation was an hour and a half out. As I looked at it, it looked very like a satellite not very far from the ring. It seems now that Mr. Common is just as much out in his observation as I was in mine, made a year and a half ago, and I, therefore, come to the conclusion that Mimas is not such a difficult object, but that with a perfectly clear atmosphere and a 6½-inch telescope of considerable excellence it may be readily seen; and when we consider that it was seen with Sir William Herschel's telescopes, I think that there can be little doubt that it would be easily seen with a modern 9 or 10-inch, to say nothing of a 15-inch.

Mr. Marth: Mr. Neison's observation was made in 1878, when several other observations and my ephemeris only differed by about 8°, which amounts to no more than half an hour in time. Now there is a difference of more than 30°, and the difference between the two sets of observations is something like 22°. I have tried again to see if I have made a mistake of a day.

The daily motion of Mimas is 382°, so that a difference of 22° would about correspond to an error of one day in the date of the observations, but unfortunately I cannot find any mistake of a day which I may have made.

Mr. Sadler: With regard to the aperture with which observations of Mimas have been made, it has, I think, been said to have been seen at Rome with a 6¼-inch refractor, and at Madras with Capt. Jacob's telescope of, I think, 6¼-in.

Mr. Marth: A single observation is not of much use, as you cannot be certain that it is not a star which has been observed. There ought to be a series of careful observations. It is impossible to reconcile the several sets of observations with any kind of motion of a satellite, but there is the possibility of some twin-brother of Mimas, and I should strongly recommend observers to look sharp after him.

Mr. Neison: If I had seen the satellite in the predicted place I might have doubted it (laughter), but having seen it an hour and a half out of its place, and that happening to be the same error as has been shown by Mr. Common, I think the improbability of its being merely accidental is very great.

Mr. Campbell: It would be unfair if I were not to confirm Mr. Neison's statement. Certainly neither of us expected to see Meimas there. We both independently drew its place on paper. Mr. Neison has put it on the black board as I saw it, he drew a little higher up and a little nearer the ring. But I did not believe that the telescope could show it, and I am not at all convinced that it did. (Laughter.)

Mr. Marth: Observers whenever they think that they have seen Mimas, ought to try and observe an hour later, and give a diagram or measurements, in order to show clearly that the body is moving as it ought to move. In the ephemerides for the last three years I have regularly given the changes of position of stars.

Mr. Common: This is an important question touching the credibility of observers. (Laughter.) I quite agree with a gentleman who recently wrote that astronomers were very loose in their description of astronomical phenomena. In 1878 the ring around Saturn was not so open an ellipse as Mr. Neison has drawn on the black board, and the orbit of Mimas would have been considerably less open. I have observed Hyperion several times, but I never had an observation of Mimas with an 18-inch telescope although I made an observation of the four satellites of Uranus, but I do not think any 6-inch telescope would touch it, and I have the strongest reason for agreeing with Mr. Marth, that no 6-inch telescope will show Mimas.

The President: I am sure the Society will vote their thanks to Mr. Common for his paper. I have a little paper myself on the satellites of Mars which have been observed at Dun Echt from Sept. the 11th.

Mr. Christie: With regard to a remark of Mr. Common's as to the use of a dark glass, the suggestion was made by me simply because it had been used with success at the Cambridge Observatory, Harvard College. I wish to mention that it was not my own suggestion. It was used by Prof. Pickering in 1877.

Mr. Common read an extract from his paper showing the way in which he had used the dark glass with a micrometer and three parallel wires, two placed on either side of the satellite. At the request of the President he also exhibited a photograph of Jupiter and the four satellites. It was taken in the principal focus of his instrument and magnified five times.

Mr. Christie: With reference to the system of three wires recommended by Mr. Common, we have used it at Greenwich and have found it very useful, particularly in the case of satellites. It gives much more accurate results.

Lord Lindsay read a note *On the spectrum of the red spot now visible on Jupiter*. At the last meeting a question had been asked as to whether anyone had examined the spectrum of the red spot, and he was not aware at the time that it had been examined at Dun Echt. On the 11th November Dr. Copeland examined the spectrum of Jupiter with the Grubb spectroscope on his 15-in. achromatic, and reported that about 100 solar lines were visible, the three magnesium lines being beautifully separated. When the slit was placed at right angles to the belts a dark band was seen corresponding to the place of the belts running from the extreme edge of the spectrum to between F and G, to a place corresponding to wave-length 453mmm. With the slit of the spectroscope parallel to the belts, a band of obscuration was seen corresponding to the detached red spot, to which so many observers had called attention. The band of obscuration seemed to be more restricted to the region of the spectrum near *b* and F than on the previous observation.

Mr. Christie: Perhaps I may mention that Mr. Maunder has also examined the spectrum of the red spot, but was not able to detect anything beyond the obscuration of the whole spectrum. There were not any bright lines.

Capt. Noble: I should like to say, with reference to some of the discrepancies between the description of the colour of the spot, that I have observed it several times since the last meeting, and have noticed that the spot seems brighter and more scarlet in twilight than when it gets darker. As the evening darkens it gets more of a maron colour.

The President: Mr. Christie will perhaps try the spectroscope on the third satellite and see what he gets.

Mr. Christie: I shall be very happy, my lord, to try that.

Mr. Ranyard read a letter from Father Perry in which he stated that, while observing a transit of the third satellite of Jupiter, he noticed that it appeared fainter than the others, and he was anxious to know whether any other observers had noticed any variation in the light of the third satellite.

Mr. Sadler said that spots had been seen on all the satellites by the late Father Secchi.

Mr. Common: There is one thing to be noted about the photograph of Jupiter I have shown. It was pointed out to me by Mr. Knobel, that the first satellite was very much fainter than the second, which is close by, and it is also fainter than the third. Perhaps that may throw a little light on the matter because there is no question of the photograph erring, as far as regards the measures of the actinic power of the light. The first satellite is much fainter than the second and third, and is very like the fourth.

Mr. Ranyard: As far as I remember, Zöllner found very satisfactory evidence of the change in brightness of the satellites, as they pass round different parts of their orbits.

Mr. Downing read a paper *On the north polar distances of the Greenwich seven years' catalogue of 1860 as compared with the Cape catalogue.* He stated that his paper showed that there was an outstanding correction to the Greenwich N.P.D.'s between the limits of R.A. 12h.—18h. amounting to $+0''\cdot26$; and if this was due to the effect of refraction it showed that the refractions used at Greenwich were too small during the months of May, June, and July. This result is confirmed by the comparison of the Cape and Melbourne N.P.D.'s between the same limits, as the resulting correction to the Cape comes out $0''\cdot01$. The corrections to the Greenwich observations at other seasons of the year are so small that they may be considered to be merely accidental.

Mr. Stone said: It appears to be pretty much the same kind of result as that which I brought before the last meeting. It is a misconception, however, to suppose that the errors were asserted to arise from the Cape catalogue alone. All I said was that there are differences between the Cape catalogue and the *Nautical Almanac* which prove that the reversion of the seasons is not completely accounted for by the refraction tables used. One point I wish to draw attention to is that there is a necessity for a caution as to the Melbourne observations, which have been made to agree fairly with the results of the northern observations by the correction of the refraction tables.

Mr. Downing: In my comparison of the Cape and Melbourne catalogues, I show that when we get near the horizon, the Melbourne catalogue north polar distances are in error, so that the refractions used at Melbourne are not correct. In the comparisons I exhibit to-night I only use the stars within 15° of the zenith of the Cape, and within 20° of the zenith of Melbourne, so that it is not likely that an error in the refractions at the Melbourne observatory can influence the result. Another point is that the corrections of Bessel's mean refractions do not represent the Melbourne observations, but agree with the results of northern observations, as I have shown in the comparison between the Melbourne and Cape places.

Mr. Marth: I should like to ask Mr. Stone whether he will be so kind as to publish the details of his investigation, so that it may be known what are the foundations of it. Hitherto only an abstract has been published in the *Monthly Notices*, which does not allow proper examination. If Mr. Stone would be good enough to give the detail of what observations he has used, how he arrived at the corrections employed, it would be of help for getting at the truth.

Mr. Stone: I am glad you have asked the question; so far as I am concerned, I can explain directly. At that time the Greenwich refractions were computed down to 82° zenith distance, with one set of refraction tables, from 82° to 85° they were computed with the reduced mean refraction; below 85° they were again reduced. Well, I must confess I consider such a proceeding as most unsatisfactory. The enquiry that presented itself to my mind was simply this. I do not care about small quantities, but we have large quantities to deal with. Is it true we have to introduce these three breaks in our refraction tables in passing from the zenith to the horizon, or not? I was by no means certain whether such would prove to be the case; but, if you look at Bessel's theory, practically it is a method of great force, for he has two arbitrary constants, and uses one of these to make refractions near the horizon agree pretty well with the observations; and the other one is afterwards used to make the mean refractions agree. Now, if it turns out that his refraction tables thus formed do represent the refractions throughout in a satisfactory manner, his assumption is a legitimate one, and no objection can be urged against his method; but supposing it turns out that you find that his refractions go on pretty well till we come to 85°, where they are too large, and require correction and alteration, then his assumption is altogether illegitimate; and either he should have pushed his assumptions sufficiently far by fresh constants to carry him down to the horizon, or have determined his second constant to make his observations at 85° agree, and so have made a continuous chain throughout; but if he used up one of his constants to make the observations near the horizon conformable, he cannot get over the difficulty by making an arbitrary break when you come to 85° from the zenith and another break at 82°, and here we see the looseness of the method. Well, I took simply the observations below the pole, made at Greenwich during the years I have given. There may be, for anything I know, difficulties in the way, but I think I may safely say that if your own circle will not give you observations sufficiently accurate to compare and determine the refractions from, you cannot expect that your circle compared with any other circle is going to give you better results. Bessel's refraction tables are based on observations of Bradley's time, and I am not prepared to admit that our observations are worse than he used. The observations I used gave the diminution of refraction mentioned in my paper. I have compared them since and they give that diminution again, and I think I am justified in saying that Mr. Christie has also compared them and that he agrees with me. When you come to the small corrections you are speaking of, you cannot separate your latitude from the refractions used. Another thing is that there are a certain number of small systematic errors in every circle that we know of. The Washington observations I suppose are considered to be good, but certainly there are indications of systematic errors in the Washington observations. There are also indications of systematic errors at Greenwich, and though I did every possible thing to avoid systematic errors at the Cape, I know I did not succeed. There are small systematic errors in nearly all these large circles; and if you get systematic errors creeping in on any important point, you are very likely to get very uncertain results. If it should turn out that these observations above and below the pole during the years I am discussing give a general diminution of refraction, and yet that result is not correct, then some systematic errors must exist in the Greenwich results. But the point I want to impress on Mr. Marth is that if he simply takes the observations above and below the pole; during the interval we are discussing they can be reconciled with diminished refractions. If the result which I arrived at then had been confirmed then we should have got over this difficulty of the break that has been introduced into the refraction tables. I have not introduced the break, and when the next catalogue is published you will see the Cape observations without a break. I do not think it is necessary for me to republish the paper Mr. Marth speaks of because I have verified it, and Mr. Christie has verified it, and anyone who takes the trouble can verify it in a few hours.

Mr. Christie : The same desire occurred to me as to Mr. Marth, to see Mr. Stone's paper, but I have since gone over the matter, in a slightly different way from Mr. Stone, and I have got substantially the same results, and I think there is no doubt that from observations of those particular years, treated above and below the pole, you must get the same result. Of course the question arises whether the certainty of observation is sufficient to justify the assumption of a change in the refraction, but that is quite another point. On that point I am rather at issue with Mr. Stone, because during those years I find the observations of the sun, which give the position of the equator, show that the change of the refractions ought to be in the opposite direction, and if you diminish the refraction and diminish the co-latitude, you will throw the position of the equator very much out. That is always the difficulty one has to deal with, namely, to reconcile the observations north of the zenith and south of the zenith. These things get so mixed up with latitude and other things, that it is very difficult to settle anything positively when you are dealing with small quantities, because the systematic errors of the circle come in, as Mr. Stone has pointed out. I think myself that the systematic errors in all circles are very much larger than most of us suppose.

Mr. Marth : I am glad that this subject will be considered. It is, of course, far too wide to be entered upon now. I will make only one remark with regard to Bessel. When Bessel undertook the reduction of the Greenwich observations, he had such boundless admiration for Bradley that he took it for granted that the observations near the horizon, though very limited in number, must be represented by his theory. When he published his first refraction tables in 1813, he had never made meridional observations himself. It was only after he had determined the refractions from his own observations, and after further observations near the horizon, that he came to the conclusion to abandon his theory beyond 85°. If Mr. Stone will publish sufficient detail to show us what observations are the foundation of his investigation, I may, probably, be able to help in discussing it.

Mr. Stone : You have not taken hold of the point. I quite understand that Bessel gives up his refraction tables below 85°, and it could not have been done if he had used but one of his constants. That is just the objection I took.

On the motion of Sir Erasmus Ommanney, seconded by Mr. Stone, Mr. Lecky, Mr. J. Campbell and Mr. Sydney Waters were appointed auditors of the accounts of the Society for the expiring year.

The following papers were also laid on the table and partly read :

R. L. J. Ellery: *Occultation of* 64 *Aquarii by Jupiter, observed at the Melbourne Observatory.*

R. H. M. Bosanquet and A. H. Sayce: *The Babylonian Astronomy, No.* 2.

S. W. Burnham: *List of New Double Stars.*

A. de Gasparis: *Sur la variation du demi-grand axe des orbites planétaires.*

Rev. S. J. Perry: *Occultation of stars by the moon and phenomena of Jupiter's satellites, observed at the Stonyhurst Observatory.*

Rev. S. J. Perry: *The November Meteors.*

W. T. Lynn: *On recent changes in the mean error of longitude of Hansen's lunar tables.*

A. Marth: *Ephemeris for finding the positions of the satellites of Uranus.* 1880.

A. Winnecke: *Note on Mr. Webb's new nebula.*

The meeting adjourned at ten o'clock.

REVIEWS.

Double Star observations, made in 1877-8, *at Chicago, with the* 18½*-inch Refractor of the Dearborn Observatory.* Comprising: I. A Catalogue of 251 new double stars with measures; II. Micrometrical measures of 500 double stars. By Sherburne Wesley Burnham, M.A. Reprinted from the Memoirs of the Royal Astronomical Society, vol. xliv. pp. 167.

The massive tower, 90-feet high, of the Chicago Observatory was built by the Hon. J. Young Scammon, a great benefactor of science and art, at the cost of 30,000 dollars, the expense of the telescope and mounting was 18,000 dollars, defrayed by subscription. There is also a meridian circle of 6-inches aperture by Repsold. Most of the measurements in this work were made with powers of 390, 638, and 925. With the highest, under favourable conditions, the glass will fairly separate two equal stars with a distance of 0″·25. In spite of much unfavourable weather, and the instrument being at the service of visitors two evenings each week, Mr. Burnham has accomplished a vast amount of valuable work, as this volume abundantly testifies. We can well believe that it has been "wholly a labour of love." Otherwise fully occupied during the business hours of every day, his observations have been prosecuted often at the expense of rest, sleep, and recreation. The results are presented to the Royal Astronomical Society as the first contribution of the great Equatorial of the Dearborn Observatory. And it is a rich contribution. It extends the number of new pairs discovered by the author to 733. Of the 251 stars in this list, 75 pairs are from 0″ to 1″ in distance, and 59 pairs from 1″ to 2″. With the measures of 500 double stars already known, the total amounts to more than 1400 micrometrical measurements. We trust that this zealous and able observer, one so eminently distinguished in this department, may be enabled for a long time to prosecute his labours, and turn to the best account—no one could do this better—the power of the magnificent instrument at his disposal. We can only note a few interesting particulars:

β Orionis (Rigel). An elongation in the 9 m. companion was suspected, as long ago as 1871, with the 6-in. refractor, and also by Mr. H. Sadler independently. In 1877 the highest powers of the 18½-in. gave a small but certain elongation. Mr. Burnham spent more time in obtaining this result than on any other star in the heavens, although some of the pairs required attention for a year or more before certainty could be obtained. The true central distance is certainly less than 0″·2. "It is the closest and perhaps most difficult pair of this class I have ever seen."

679, *anonymous*. R.A., 1880, 21h. 1m. 25s. Dec. 43° 12′.

Position angle 71°·5, distance 0·3″. ± epoch 1870 +7·556
" " 64°·7 " 0·38 " " +8·647

"Without doubt this is the most minute close pair known, and is worth noting as a curiosity in double stars, if for no other reason. . . The magnitude of the double would be perhaps more nearly 11 than 10. It is near ξ Cygni."

*θ*¹ *Orionis*. About the supposed additional stars, besides the fifth and sixth in the trapezium in Orion, Mr. Burnham says, "I have very little faith in the real existence of these suspected stars, after the failure of this and other large refractors to show them. It is wholly improbable that they should all be variable in such a manner as to render them at all times invisible within the last few years." He also finds no evidence of the variability of the fifth and sixth stars.

70 *Ophiuchi*. "Jacob suggested the existence of a third star, by way of explaining some apparent irregularities in the orbital motion, and some observers have suspected an elongation of one of the components. Both stars were perfectly round with the highest powers, . . as they have always been to me heretofore, and no trace of any third star near."

α Lyræ. "I have examined *α Lyræ* once under very favourable conditions with the Washington 26-inch, when the star was near the meridian, and many times with the 18½-inch, but have never seen the slightest trace of the close stars supposed to have been seen by Buckingham and others" (*Monthly Notices*, Nov., 1867). These were certainly not real stars, but optical illusions of some kind, due to the observer or the instrument. The object-glasses of Alvan-Clark and Son rarely give these spurious images in the vicinity of bright stars.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE NOVEMBER LEONIDS, 1879.

Sir,—Mr. Gray in his interesting notes of the "November Meteors of 1879," (*Register*, No. 104, p. 297) remarks that those who expected only some portion of the usual meteoric shower on the 13th [of November] must have been greatly disappointed.

I was making observations on the night in question, and saw 100 shooting stars, of which 18 were brilliant Leonids. Twelve of these were recorded between 14½h. and 15½h., when the display appears to have reached its maximum, and the radiant point was very sharply defined at 148° + 23° from the following tracks.

Paths of Leonids seen at Bristol, November 13, 1879.

Time.		Mag.	From		To		Path.	
h.	m.		°	°	°	°		
11	9	1	10	+ 12	48½	— 4	43	not swift, streak.
13	7	2	80	+ 10	58	+ 1½	23	meteors all very swift with bright lingering streaks.
14	28	2	128	+ 39	116	+ 45	11	
14	33	5	100	+ 38½	79	+ 39	17	
14	41	3	105	+ 30	79	+ 29	22½	
14	44	3	150	+ 37	151	+ 41¾	5	
14	58	2	182	+ 68	224	+ 75	14½	
14	59	2	151	+ 49	153½	+ 60	11	
15	1	3	124	+ 2	119	— 3	7	
15	6	=♃	98	+ 37	77	+ 36	17	
15	13	=♂	153	+ 34	157	+ 39	5½	
15	19	2	141	— 3	138¾	— 12	9	
15	28	1	119	+ 14	107	+ 9½	13	
15	34	4	145	+ 8	144	+ 3	5	
15	54	3	168	+ 44	182	+ 51	12	
16	16	=♂	187	+ 42	205	+ 44½	13	
16	41	=♃	184	+ 30	193	+ 30	7½	

Mr. Gray mentions a conspicuous meteor at 10h. 20m., on Nov. 12. This seems to have been observed by Mr. Corder at Writtle and by myself at Bristol. It was undoubtedly a Leonid, though the radiant point had not nearly risen, and the remarkable features of its appearance were the very bright streak in left and the long path traversed. Mr. Corder estimated the length as 50°, but he did not see the beginning. At Bristol the meteor began at 98° + 22°, and ended at about 4° — 15°, length of path = 98°. With a single exception this is the longest course recorded by me out of an aggregate of more than 5,700 shooting stars observed here during the last four years. The meteor pursued its track with a slow motion, the duration being estimated by Mr. Gray as five seconds, and by me as four seconds. Mr. Corder says it "travelled very slowly." Another meteor, very similar to the one described, was seen at 11h. 9m., on Nov. 13, and a singular path was noted the same night at 15h. 31m., rising upwards from slightly west of α Leonis towards

Polaris, with a length of 46°, in which there was a slender streak all the way.

The recent shower of Leonids was certainly more decided than during any of the few preceding years, and the display was evidently sustained on the five nights, from the 11th to the 15th inclusive. On the latter date Mr. Sawyer at Cambridgeport, Mass., saw 16 meteors (13h. to 15h.), of which seven were Leonids, with a radiant point at 151° + 22°. This is close to the position assigned by Mr. Corder (149° + 23°) and myself (148° + 23°). The first indication of the shower was given by a bright meteor at about 11h. 31m., on Nov. 11, observed at Greenwich and Writtle, amongst the stars of Eridanus and Cetus. The maximum of the shower, as observed by Mr. Corder, was on the morning of the 13th, when 12 of its members were recorded, and on the two following mornings the continuance of the display was traced by further meteors. Out of a total of 29 paths conforming to this stream, no less than eight were those of first mag. meteors, and 18 left streaks.

Ashley Down, Bristol: I am, Sir, yours faithfully,
Dec. 10, 1879. W. F. DENNING.

ABNORMAL FORM OF THE SHADOW OF JUPITER'S SATELLITE III.

Sir,—While observing Jupiter in 1879, July 26th, under good definition and power of 270, at 11h. 50m., I first noticed the shadow of the satellite, which appeared then about half on the limb. At 12h. 5m. it was completely within the limb, and although it was not the object of my examination of Jupiter, it forced from me some attention by its unusual form. It was not round, but oval, the elongation being N. and S. It continued of that form until 12h. 40m., when it had become more round, having, during the interval, slowly and gradually approached its normal appearance.

I am always doubtful of the reality of peculiar observations of this kind, and am inclined to think they are more often subjective than objective, but as I could see no way of accounting for this one it seemed perhaps worthy of record.

Yours truly,
HENRY PRATT.

THE RED SPOT ON JUPITER.

Sir,—Last evening (the 11th) I examined Jupiter, with special reference to the Rev. Mr. Webb's observation of the red spot on

Nov. 12th, when he thought he saw in it signs of a tendency to separation into two parts.

I observed from 5h. 10m. to 5h. 30m., using 8½ inches of my 10 inch Calver mirror, and powers of ±150 and ±220. There was a good deal of fog about, but the sky was almost cloudless, and definition at times very good. The spot was well on the disk, and I may say at once that I could find no trace of this appearance of separation. The southern boundary of the spot might have been perhaps less curved, and the whole spot more elongated, and less truly oval shaped than in my August and September observations, also the W. or *p* end seemed now sharper and more elongated. The surroundings of the spot, too, appeared modified since then. I did not notice the *pushing aside* by the spot of the belts, &c., which has been formerly clearly seen, and which was referred to by Captain Noble at the November meeting of the Royal Astronomical Society, but the spot seemed to lie on a broad white band which did not appear to me to brighten in the immediate vicinity of the spot. The colour of the latter did not look the same as that of the north equatorial belt, which was a deep orange red, the spot was a kind of red brick-dust colour, but I cannot find any comparison which exactly represents it.

The major axis of this spot seemed to me not quite parallel to the general direction of the belts, the west end lying very slightly more south than the other. At times I thought I caught traces of darkish markings on the spot, but this is very uncertain. I estimated the time of passage of the west end over the central meridian as about 5h. 22m.

I am, Sir, faithfully yours,
Rathfern, Putney Hill, S.W.: JAMES L. McCANCE.
1879, Dec. 12.

NEW GASEOUS NEBULA.

Sir,—On the night of Nov. 14, as I was sweeping among the glories of Cygnus, with a power of probably about 50 on my beautiful 9·38-in. With mirror, I came across an object like a bluish 9 mag. star; on closer attention, however, not quite of a stellar character. Thinking that it might prove to be a small pair, too close to be separated with that eye-piece, I changed it successively for 212, 375, and 450, with each of which it was found to be a bright, very ill-defined, nebulous disk, of about 4″ in diameter, surrounded probably by a little glow, and much resembling the planet Uranus in an inferior instrument and especially bad atmosphere. I have since had the pleasure of finding it identified by Lord Lindsay and Mr. Knott as Argelander +41° No. 4004, XXIh. 1m. 35·2s., N. 41° 39′·3 (epoch 1855), and they concur in the discovery that it shines by monochromatic light. As far as I can recollect the planetary nebula Σ5, which I have not seen for many years, the present object greatly resembles it in character, though on a much smaller scale. Any of your readers who may feel disposed to search for it must of course employ a sufficient power to bring out the minute soft disk. It is the brightest object in a field of 36′, a rather dull region as compared with the splendours of the neighbourhood.

Hardwick Vicarage: Yours faithfully,
29 Nov., 1879. THOMAS WILLIAM WEBB.

LUNAR CORONA.

Sir,—I have read Dr. Berwick's letter, and would suggest that he should make a longer series of experiments before he assumes that the photographic halo can only be obtained with the full moon. The diameter of the moon's image given by his camera is probably small compared with the thickness of the glass plate upon which his photographs are taken, and I am inclined to attribute his photographic halo to reflection of the moon's rays from the back of the plate.

A few years ago I obtained a similar halo by photographing a small aperture in a screen in front of a gas lamp; the inner diameter of the ring varied with the thickness of the photographic plate, and appeared to correspond with the angle at which rays would be *totally* reflected from the back of the plate.

It seems that light falling on the collodion film is dispersed in all directions. The rays, falling nearly perpendicularly upon the back surface of the photographic plate, emerge with but slight reflection, while those which fall more obliquely on the back surface are totally reflected. There is thus round every luminous point on the collodion film a ring where the intensity of the rays reflected from the back surface of the glass is greatest.

The full moon gives more light than the moon in her other phases, and I conclude that with longer exposures Dr. Berwick will be able to obtain a nebulous ring round the image of the moon in her other phases.

Yours faithfully,
A. C. RANYARD.

ASTRONOMICAL OCCURRENCES FOR JANUARY, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars.
Thur	1		Sidereal Time at Mean Noon 18h. 42m. 6·56s.	1st Oc. D. 1st Ec. R.	4 22 7 47 3	8 3·3
Fri	2		Sun's Meridian Passage 3m. 37·78s. after Mean Noon	1st Sh. E.	4 58	8 0·1
Sat	3	13 12 13 49	Occultation of B.A.C. 4006 (6) Reappearance of ditto			7 57·0
Sun	4	18 48	☾ Moon's Last Quarter	2nd Tr. I.	8 24	7 55·9
Mon	5					7 53·9
Tues	6			2nd Ec. R.	7 37 37	7 47·9
Wed	7	19 10 20 15	Occultation of B.A.C. 5197 (6) Reappearance of ditto	3rd Tr. E 3rd Sh. I.	7 31 8 27	7 44·9
Thur	8	8	Conjunction of Moon and Venus 6° 6′ N.	1st Oc. D.	6 22	7 42·0
Fri	9	23	Conjunction of Moon and Mercury 1° 47′ N.	1st Sh. I. 1st Tr. E. 1st Sh. E.	4 36 5 51 6 53	7 39·1
Sat	10			1st Ec. R.	4 11 10	7 36·2
Sun	11	10 39	● New Moon Eclipse of the Sun invisible at Greenwich			7 33·4
Mon	12					7 30·6
Tues	13		Sidereal Time at Mean Noon 19h. 29m. 25·25s	2nd Oc. D.	5 25	7 27·8
Wed	14		Sun's Meridian Passage 9m. 11·39s. after Mean Noon	3rd Tr. I.	8 23	7 25·1
Thur	15	6	Conjunction of Moon and Jupiter 6° 5′ S. Saturn's Ring: Major axis=39″·08 Minor axis= 5″·35 Illuminated portion of disk of Venus=0·684 Illuminated portion of disk of Mars=0·908	2nd Sh. E. 1st Oc. D.	5 24 8 23	7 22·4
Fri	16	5 10 6 21	Occultation of 19 Piscium (6) Reappearance of ditto	1st Tr. I. 1st Sh. I. 1st Tr. E.	5 33 6 32 7 52	7 19·8

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars.
Sat	17	13	Conjunction of Moon and Saturn 8° 21′ S.	4th Ec. D. 1st Ec. R. 4th Ec. R.	5 11 38 6 6 24 8 25 47	— 7 17·1
Sun	18	8 40	☽ Moon's First Quarter	3rd Ec. R.	5 40 19	7 14·5
Mon	19					7 12·0
Tues	20	7 4 8 25 15	Occultation of ε Arietis (4½) Reappearance of ditto Conjunction of Moon and Mars 2° 31′ S.	2nd Oc. D.	8 15	7 9·4
Wed	21	15 27	Near approach of 36 Tauri (6)			7 6·9
Thur	22	15 5 15 43 16 9	Occultation of B.A.C. 1518 (6) Reappearance of ditto Near approach of *k* Tauri (5½)	1st Sh. I. 2nd Tr. E. 2nd Sh. E.	5 11 6 14 8 0	7 4·4
Fri	23	4 11 14 48 15 97	Near approach of 118 Tauri (6) Occultation of 132 Tauri (5½) Reappearance of ditto	1st Tr. I.	7 34	7 1·9
Sat	24			1st Oc. D. 1st Ec. R.	4 54 8 1 32	6 59·5
Sun	25	11 1	Near approach of 63 Geminorum (5½)	1st Sh. E. 3rd Oc. R. 3rd Ec. D. 4th Tr. I.	5 14 6 28 6 33 20 7 44	6 57·1
Mon	26	22 12 14 49 15 43	○ Full Moon Occultation of d^2 Cancri (6) Reappearance of ditto			6 54·7
Tues	27					6 52·3
Wed	28	10 8 11 15 11 29 12 37	Occultation of B.A.C. 3407 (6) Reappearance of ditto Occultation of π Leonis (5) Reappearance of ditto			6 50·0
Thur	29	10 12 7	Near approach of 36 Sextantis (6) Neptune at quadrature with the Sun	2nd Tr. I. 2nd Sh. I.	6 11 7 48	6 47·7
Fri	30					6 45·4
Sat	31			2nd Ec. R. 1st Oc. D.	4 51 28 6 56	6 43·1
FEB. *Sun*	1	17 28	Near approach of 69 Virginis (5½)	1st Sh. I. 3rd Tr. E. 1st Sh. E. 3rd Oc. D.	4 52 6 25 7 9 7 26	6 40·9

THE PLANETS FOR JANUARY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	17 13 34	S. 21 33½	6″·2	22 27·8
	9th	17 57 39	S. 23 8	5″·6	22 40.3
	17th	18 47 38	S. 23 41	5″·2	22 58·7
	25th	19 40 42	S. 22 55½	4″·8	23 20·2
Venus ...	1st	15 40 17	S. 16 47	18″·6	20 54·8
	9th	16 18 11	S. 18 49½	17″·4	21 1·1
	17th	16 57 28	S. 20 25½	16″·4	21 8·8
	25th	17 37 53	S. 21 29	15″·6	21 7·6
Mars ...	1st	2 46 42	N. 17 56	14″·6	8 3·3
	9th	2 53 59	N. 18 33	13″·4	7 39·1
	17th	3 3 31	N. 19 4½	12″·6	7 17·1
	25th	3 14 58	N. 20 2½	11″·4	6 57·1
Saturn ...	1st	0 38 15	N. 1 25½	16″·0	5 55·2
	9th	0 39 29	N. 1 35½	15″·8	5 24·9
	17th	0 41 6	N. 1 48	15″·6	4 55·1
	25th	0 43 5	N. 2 3	15″·4	4 25·6
Uranus ...	17th	10 41 40	N. 9 8	4″·2	14 54·1
Neptune ...	1st	2 30 11	N. 12 54	...	7 46·8
	17th	2 29 48	N. 12 53	...	6 43·5

Mercury rises an hour and three-quarters before the sun on the 1st, the interval decreasing.

Venus rises nearly four hours before the sun, at the beginning of the month, the interval decreasing.

Mars sets three hours and three-quarters after midnight on the 1st, the interval decreasing.

Saturn sets a few minutes after midnight on the 1st, and then earlier each night.

SATELLITES OF URANUS.

Major and minor semi-axes *a* and *b* of the apparent ellipses described by the satellites, and position-angles p_0 of the major axes:

	Ariel.		Umbriel.		Titania.		Oberon.		
1880.	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	p_0
	″	″	″	″	″	″	″	″	°
Jan. 15	15·05	1·88	20·96	2·60	34·38	4·25	45·98	5·72	14·03
25	15·14	1·97	21·09	2·74	34·60	4·49	46·27	6·01	13·97

In these apparent orbits the satellites move in the direction of decreasing position-angles, from their greatest northern elongation N. (in position angle 14°·0) to inferior conjunction (pos. 284°·0), greatest southern elongation S. (pos. 194°·0), superior conjunction (pos. 104°·0), etc. The satellites will be at these points of their orbits about the following hours, G.M.T.:

Ariel.			Umbriel.			Titania.			Oberon.		
1880.	h.			h.			h.			h.	
Jan. 15	2	N.	Jan. 16	2	N.	Jan. 15	13	sup.	Jan. 14	13	inf.
16	8	S.	18	4	S.	17	17	N.	17	22	S.
17	15	N.	20	6	N.	19	21	inf.	21	6	sup.
18	21	S.	22	7	S.	22	1	S.	24	15	N.
20	3	N.	24	9	N.	24	6	sup.	28	0	inf.
21	9	S.	26	11	S.	26	10	N.	31	9	S.
22	16	N.	28	13	N.	28	14	inf.	Ariel.		
23	22	S.	30	14	S.	30	18	S.	28	23	S.
25	4	N.	Feb. 1	16	N.	Feb. 1	18	sup.	30	5	N.
26	10	S.							31	11	S.
27	17	N.							Feb. 1	18	N.

A.M.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JANUARY, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Plato.—But little attention has been bestowed on Plato during the last seven years, or since the discussion of the observations of the spots and streaks on the floor, which formed the subject of two reports in the volumes of Reports of the British Association for the Advancement of Science for 1871 and 1872. Facing page 63 of the report for 1871 is a table of the visibilities of the 36 spots then known, arranged in groups of intervals of twelve hours each preceding and succeeding the sun's meridian passage at Plato, the visibility being expressed in decimals of the standard visibility 1·00 of spot No. 1, which was seen on almost every occasion. On the evenings of November 29 and 30 no less than *thirteen* of the 36 spots were seen, *nine* by Mr. Harding of Bournemouth; on the 29th between 12h. and 13h. 30m., viz., spots 1, 4, 3, 17, 5, 14, 16, 13 and 24. Mr. Allison of Chesterfield on the same evening saw in addition to 1, 4, 3 and 14, the spots 22, 12, 19, making *seven* in all; the spots 17, 5, 16, 13 and 24 he did not see. On the 30th of November Mr. Gray of Bedford saw *five* spots, Nos. 1, 2, 4, 14, common to the series, Harding and Allison, and one not seen by either, No. 15, so that on the two evenings *thirteen* of the spots on Plato were seen by *three* observers. Mr. Allison describes 1 and 4 very clear, 3 hardly seen, 14 a bright patch, also a bright patch about 12 and 19, and 22 was occasionally seen as a bright patch.

Spot

No. 1=1·00 H A G	No. 17=·81 H	No. 22=·43 A	No. 19=·24 A
4= ·86 H A G	5=·57 H	16=·33 H	15=·09 G
3× ·81 H A G	14=·43 H G A	13=·29 H	24=·05 H
			12=·05 A

The visibility in the above table are those determined in 1871 for a period of 120 to 60 hours before sunset at Plato, the letters being the initials of the observers, A, Allison, G, Gray, and H, Harding.

To ascertain how far the visibility of the thirteen spots may have depended on the illuminating angle, the epochs of the return of similar phase are appended. Jan. 27, 11.30, and Jan. 28, 17.13, respectively.

Proclus.—In our list for October, 1879, see *ante*, page 242. We solicited attention to Mr. Dennett's diagram of the lucid streaks in the neighbourhood of this crater. In this diagram, *English Mechanic*, No. 754, page 648, three streaks emanated from a point N.W. of *Proclus* VII., VIII. and X. On the 24th Nov., 1879, Mr. Allison of Chesterfield ascertained the existence of a fourth streak which emanated from the same centre, crossed VIII. and proceeded south-westwardly between Pierce and Picard E, its southern edge appeared to touch the northern wall of E. This streak was also seen by Mr. W. Harding of Bournemouth on Nov. 25th, 1879.

Water Lane, Stratford, E.:
December 20, 1879.

Book received.—Lecture on Practical Astronomy. By Rev. J. Challis, M.A., F.R.S., F.R.A.S. Cambridge: Deighton, Bell & Co. 1879.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1879.
Ennis, J.
Hargreaves, T.
Lawton, W. E.

To Jan., 1880.
Denning, W. F.

To March, 1880.
Berwick, Dr.

To June, 1880.
Banks, Mrs.
Rivaz, Miss.

To Sept., 1880.
Hemming, Rev. B. F.

To Dec., 1880.
Arcimis, A. T.
Barber, J. T.
Baldelli, Contessa.
Boë, A. de.
Freeman, G. T.
Fleming, Rev. D.
Franks, W. S.
Johnson, Rev. S. J.
Jones, Rev. E.
Joynson, J.
Knott, G.
Numsen, W. A.
Rogerson, G.
Tidmarsh, Rev. J. B.
Walker, G. J.
Witty, E. A.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to John C. Jackson, at Lower Clapton, London, E.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 206. FEBRUARY. 1880.

ROYAL ASTRONOMICAL SOCIETY.

Session 1879—80.

The last meeting of the Session was held at the Society's rooms on Friday, the 9th January, 1880.

Lord Lindsay, M.P., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

Mr. E. A. J. Crossley, 26, Blenheim Road, N.W.
Mr. W. S. Franks, 1, High Street, Leicester.
Mr. A. Riches, Brunswick School, Leamington.
Mr. J. F. Sloman, B.A., Auckland College, New Zealand.
Rev. W. Smith, 28, St. George's Street, Chorley, Lancashire.
Senor D. Rafael Rorg y Torres, Calle de Fontanella 28, Barcelona,

were balloted for and duly elected Fellows of the Society.

Mr. Glaisher announced that since the last meeting 42 presents had been received by the Society, amongst which was Dr. Gould's "Uranometria Argentina" with a large atlas of 14 maps of the southern heavens. The thanks of the Society were returned to Dr. Gould and the other donors.

Captain Noble said: I am extremely unwilling, my lord, to intrude upon the time of the Society with a personal matter; but my excuse for doing so must be found in the fact that it is one which to a certain extent affects the dignity as well as the conduct of the meetings of the Society. I am afraid some of those present at the meeting in November will remember that in a moment of irritation I was betrayed into the use of the unparliamentary phrase, "pot-house abuse," with reference to a certain interruption. The discussions of this Society are reported in two or three astronomical journals, and among others in a publication called the *Observatory*. Mr. Christie sent me a proof of my remarks at the November meeting for revision in this paper, and through this most objectionable phrase I ran my pen, with a marginal request or note that it might be struck out; but for some reason, which no doubt Mr. Christie will be able to explain, he printed it. I much regret that he did so, as I have no hesitation whatever in saying that it was a form of expression which no amount of provocation would justify (applause), and I wish to withdraw it, and express my regret to the Society that I should have been irritated into using so very unparliamentary a form of speech. (Hear, hear.)

Mr. Ranyard read a paper by Mr. Pratt, of Brighton, *On the rotation period of the planet Jupiter*, which had been determined from a series of observations of the transit of the great red spot, which has been visible for the last few months, across the central meridian of the planet's disk. A micrometer was made use of to determine the distances of the end of the spot from the east and the west limbs, and when these distances were equal it was concluded that the middle of the spot was on the central meridian. From two series of observations Mr. Pratt deduces a rotation period of 9h. 55m. 49·3s. of mean solar time. The rotation period determined by Cassini, in 1665, was 9h. 55m. 49·5s. Since that time Maraldi, Sir W. Herschel, Schroeter, Schmidt, Mädler, and Sir G. B. Airy have all made estimates of the rotation period, the last being that of Schmidt, made in 1866, being 9h. 55m. 46·3s.

An examination of the observations of Mr. Pratt, which include 321 rotation periods of the planet, does not yield any evidence tending to prove that there is any drift of the great red spot similar to the drift observed in sun-spots, or, at all events, if there has been any such drift, it has been perfectly uniform. The rotation period which is most frequently quoted in astronomical text-books, is 9h. 55m. 26s. During the 321 rotations between the first and last observations of the series that rotation period would produce a discordance equal to 42m. 19s., and by that time not only the middle but the following end of the spot would have considerably passed the central meridian on the date of the last observation.

The President: I think the Society will pardon me for having asked Dr. Huggins to communicate to us this evening some facts with respect to the photography of the spectra of stars, which he has already sent to another society, and therefore it can only be by the invitation of the members present that he can bring them before the meeting this evening. (Hear, hear.)

Dr. Huggins: My Lord President, I have great pleasure in giving a short account of some results which I have obtained during the last four years, on the ultra-violet portion of the spectra of stars. Perhaps I may state first the general form of apparatus which has been employed. It will be obvious that it is not very easy to obtain photographs of the spectra of stars, because the light of a star when spread out by dispersion so as to form a spectrum becomes very faint, and this circumstance, taken together with the extreme desirability of using a very narrow slit for the purpose of accurately noting the position of the lines, causes the spectra to be extremely faint as compared with the light of the star. Another circumstance, as even those who are in the habit of using the best clocks will admit, increases the difficulty, and that is, that it is not an easy matter, to keep the image of a star upon a slit not wider than $\frac{1}{350}$th part of an inch, for an hour together, which often has to be done. It was, therefore, of extreme importance to determine the form of spectrum apparatus which should give a minimum spreading out of the light of the star compatible with a very detailed spectrum, that is, a spectrum containing the finer lines. After trying several different arrangements, I adopted a form of spectrum apparatus consisting of one prism of Iceland spar, which is very transparent to the ultra-violet rays, very nearly as much so as quartz, and also, as every one knows, possesses a very much greater dispersive power—so that a prism of 60° of Iceland spar is about equal to a prism of dense flint glass of the same angle. The lenses were of quartz, and the spectral apparatus was provided with a slit of the width of $\frac{1}{350}$th part of an inch. With this apparatus a spectrum was obtained of the portion of the spectrum between G (the commencement of this map) and O in the ultra-violet. The length of the photograph obtained is only half-an-inch, and yet in this small spectrum, by means of a magnifying glass, at least seven lines can be seen between the lines H and K. I mention this because I shall show presently that the nebulous appearance of the strong lines does not arise from any want of defining power of the apparatus, or from the spectrum not being sufficiently pure. It is sufficiently pure to show seven or eight lines in the solar spectrum between H and K. This spectroscope was fitted to the principal focus of the mirror of the Cassegrain telescope which belongs to the Royal Society, and is in my possession, the whole instrument consisting of an achromatic telescope of 15-inches aperture, and this Cassegrain telescope, which can be mounted on the same stand and be driven by the same clockwork. The Cassegrain telescope was employed, the small mirror being removed and the spectroscope placed so that the slit-plate should be at the principal focus of the metallic speculum of 18-inches diameter. There is also an arrangement by which the slit can be placed precisely at the focal plane; this was found to be essential to good definition, and also by which the axis of the collimator can be brought into the optical axis of the mirror. Then came the difficulty of placing the image of the star absolutely upon any part of the slit, and also of maintaining it there without allowing it to go off and take an excursion on one side or the other during the exposure of one hour. This difficulty I overcame very easily by a device which consists essentially of placing in front of the slit a silver plate, highly polished, with a small longitudinal opening rather larger than the slit itself, placed over the slit, and then in the opening of the large mirror (being the Cassegrain telescope, the large mirror has a hole at its centre) the eyepiece being removed, a small Galilean telescope was placed and focussed upon this metal plate which was illuminated by a lamp, the light of which is passed through yellow glass before it is thrown upon the silver plate, so that it produces no photographic effect.

On looking through the small Galilean telescope you see the silver plate and the slit distinctly, and also the image of the star as a bright point, and by means of the equatorial motions this bright point can be brought exactly on any part of the slit, and, by supervision during the whole time of the exposure, can be kept there, and if it has the slightest tendency to depart it can be brought back by the ordinary motions of the equatorial, the image of the star being rather wider than the slit. When the central portion of the star disk is upon the slit the light of the star can still be seen on either side.

Different photographic methods were employed, but ultimately the extreme sensitiveness of gelatine plates led to the adoption of that form of preparation. I should say the photographs are afterwards examined under the microscope, to which a micrometer is attached, giving the means of measuring with accuracy the 100th part of the distance between H and K. The position of the lines is measured by the graphic method; and making use of Cornu's map of the ultra-violet part of the spectrum, the position of the lines has been determined in wave-lengths, and they are laid down on this diagram.

[Dr. Huggins proceeded to explain the diagram which had on it the solar spectrum and below in order the spectra of Vega, Sirius, *η* Ursæ Majoris, *a* Virginis, *a* Aquilæ, *α* Cygni, and Arcturus.]

When you look at the spectra of what may be considered the typical stars of the class of white stars, for example, Vega and Sirius, you at once notice a peculiar rhythmical arrangement in the grouping of the lines. Twelve strong lines are here represented, and it will be seen that the distance between the lines decreases as the refrangibility increases, suggesting at once that this group of lines indicates one substance. The first of these lines coincides with a line of hydrogen, the second line with the small line *h*, of the solar spectrum. This line is also a line of hydrogen, and the third line with H. The line K, which is equally broad in the solar spectrum, is either absent, or represented by an exceedingly fine line which can only just be perceived in the photograph. Then you have nine lines which do not coincide with any line on the solar spectrum. Then the spectrum does not end here, but may be traced on the plates even beyond S. The group of lines present the appearance as if they belonged to one substance, and two of them belong to hydrogen. There is a line in the photographs taken of the hydrogen spectrum, which Mr. Lockyer has pointed out coincides with H, and it is in the maps of Mr. Rand Capron, and I have it in my photographs of the hydrogen spectrum; but I have been able to trace in some photographs of the hydrogen spectrum fine lines near the position of these lines, and I think, therefore, there is a very considerable probability that these lines do represent either hydrogen as we know it, or in a somewhat different condition. Then the two lines, H and K, coincide also with two strong lines in the spectrum of calcium; but there are no stellar lines co-incident with a more refrangible pair of lines of calcium. I have also here the spectra of three other stars, also of the white class—*η* Ursa Majoris, *a* Virginis, *a* Aquilæ, and *α* Cygni, I have shown them in the order in which they appear to differ from what one might call the typical spectrum. As you pass on you will see that though these lines are present they become narrower and more defined at the edges; at the same time other lines come in. In this second spectrum the line corresponding to K becomes stronger than the typical line, showing an approach towards the solar type. In the spectrum below we have that of Arcturus, which you will see resembles the solar type; but if one considers the relative thicknesses of the lines H and K, this spectrum may be considered to be on the other side of the solar type in the order of change from the typical form, the second line K being broader than it is in the solar spectrum relatively to the thickness of H. It seems by no means impossible that these various changes, in the order in which they are presented in this map, may indeed represent the order of changes in time through which any sun passes. It is by no means an impossible case that Vega and Sirius, being two of the whitest, are also in the earliest stage; Arcturus, which is yellower, may probably be beyond our sun in the order of departure from the type of Vega, whatever that may signify.

I have here a small photograph—a copy taken directly, by superposition, from one of the negatives, which can be handed round, of the two stars η Ursæ Majoris and α Cygni. The slit has two shutters, so that when a star has been taken on the plate, that shutter can be closed and the other opened and a second spectrum taken, on the same plate, so as to serve for comparison; it may be the solar spectrum, or a spectrum taken from the moon, or the spectrum of a known star; or the apparatus can be removed and a terrestrial spectrum can be taken.

With the same apparatus spectra of the planets were taken. The plan adopted was that which I employed in 1864 with the naked eye, viz., to take them before it was dark, in the afternoon when the light in the sky was fading, so that with a broad slit upon the photographic plate you have a broad spectrum corresponding with the spectrum of the sky, and a narrow and more intense spectrum which is due to the light of the planet, and in this way any difference between the light of the planet and that of the sky at the same time would be at once perceived. I have photographs of the spectrum of Venus, Mars, and Jupiter, but there is no indication of any difference or any modification of the solar spectrum between G and O. With my apparatus I have also taken spectra of small portions of the lunar surface, but hitherto without any indication of a lunar atmosphere. This apparatus, by enabling one to take a very small area, or a restricted portion of a heavenly body is specially suitable for taking photographs of the spectra of different portions of the sun's spots, which I hope shortly to do. I think it may also be possible to get lines in the ultra-violet portion of the spectrum of the gaseous nebulæ. I fear I have detained the meeting longer than I ought to have done. (Applause.)

Mr. Christie: I think there cannot be two opinions as to the importance of Dr. Huggins's work on the ultra-violet part of the spectra of stars. I must confess that when he first mentioned to me that he was taking up the question of photographing the spectra of stars, I hardly expected he would get much in the way of important results. I thought the difficulties in obtaining anything by photography would be so great that I could hardly hope that anything valuable could come out of it, but now I have great pleasure in recognising how much he has done in that respect. One important part of this is, of course, that he has extended the spectrum to a part which cannot be examined with the eye, and that is a matter of very great importance. There are other points, too. He has also got permanent records of the relative intensities of the lines, and that is a matter much neglected by observers, and in which there is a great difficulty of obtaining any certainty from mere estimations; but when you have a photograph you can measure it at leisure, and it can be reproduced, and there is something which nobody can gainsay. Then to arrange the spectra of stars in order of merit, so to speak, is certainly a very great point gained, because although Secchi had arranged the stars in types, he did not go to the extent that Dr. Huggins has done, and did not differentiate between stars of the same type—that is, stars of the white class. Now, Dr. Huggins has given us the spectra of Sirius and Vega in one class. and then we have *α* Virginis, *α* Aquilæ, and *α* Cygni in another class; and then we have the spectrum of the sun and the spectrum of Arcturus, and the very important point is brought out that there is a difference between the characteristic behaviour of the two lines supposed to be due to calcium, H and K. Of course, in these matters there may be a little difficulty in showing if the lines do actually belong to the substance or fall near to them, but that is not the point here. There is a difference in the behaviour of certain lines in the spectra of different stars. That is an important scientific fact that Dr. Huggins has established. There is one other point of interest, though Dr. Huggins has not alluded to it, and that is that in the spectrum of *α* Virginis the F line in particular is very broad. It is broader than in Vega or Sirius. At any rate it is a star the lines of which we have had the greatest difficulty in measuring, in our attempts to determine the proper motions of the stars in the line of sight, in continuation of Dr. Huggins' work—yet in this photograph of the ultra-violet it appears that this star shows a sharp line. Now, that is a very important point, it seems to me, and it is a matter that deserves some further discussion, that we should have the spectrum of a star belonging apparently to one type in the visible part, and to another type in the ultra-violet. I should like to ask one question of Dr. Huggins as regards his diagram, and that is whether the breadth of the H line (to take that as an instance) is represented relatively throughout in its proper proportions, and whether the H line is broader in the sun, and in Vega, and Sirius, and Arcturus, than in the other stars, particularly *α* Aquilæ and *α* Cygni?

Mr. Ranyard: I should like also to ask Dr. Huggins one or two questions. I think I understood him to say he found no difference in the spectrum of Jupiter. There is such a difference in the photographic action of the different parts of Jupiter—the belts and brighter parts—that one would have expected that there would have been some recognisable difference in the spectrum. Do I understand that in the whole breadth of the spectrum of the planet Dr. Huggins has detected no difference or absorption? I should like also to learn how his former observations with regard to Uranus are borne out, and whether he has been able to obtain any photographs of the spectrum of the planet? And if I may be allowed to ask a third question, I should like to hear whether his investigations have thrown any light on the question whether the broad H lines in the solar spectrum have any physical connection with the group of lines within them, or whether it seems probable that the coincidence as to position is only accidental? I conclude that none of the fine lines within the H lines would be recognisable in the stellar photographs.

Mr. Neison: I should like to ask Dr. Huggins another question. I should not have risen to say anything if Dr. Huggins had not touched upon a favourite subject of mine in regard to the moon and its atmosphere. I understood Dr. Huggins to say that he had observed the spectrum of the light from the moon, and is unable to find any indications of an atmosphere. I should like to ask whether it is not true that, supposing that the moon's atmosphere was of a similar nature to our own, we ought to expect to see anything, because the effect which that atmosphere would produce on the spectrum would be masked by the similar effect produced by the much greater amount of our own atmosphere, through which the light also passes.

In reply to the call of the President,

Mr. Rand Capron said: I have listened with much pleasure to Dr. Huggins's remarks; but I have had no experience in photographing any of the spectra of celestial bodies. I have had some experience, as possibly you know, in photographing the spectra of the metals and the gases; and with reference to one of the questions put to Dr. Huggins—that is, as to the breadth of the hydrogen lines in the spectrum—I can only venture to express an opinion that it is a question of the pressure exercised upon the gas at the time. I have not, of course, at present heard Dr. Huggins's answer as to the width of the hydrogen line as shown on the diagram, but I rather assume from what Dr. Huggins has told us that these lines are represented on the plan approximately as they appear upon the photographs—that is to say, they are wider in the sun, Vega, and Sirius than they are in the other stars. There is no other remark which at present occurs to me, except to notice the extremely ingenious method that seems to have been adopted to keep the star-spectrum upon the slit; and also—what Dr. Huggins has rather modestly overlooked—a fact that I can speak to from experience with much more manageable objects than stars, and that is the extreme difficulty of getting minutiæ of detail in spectrum photographs. I think it ought to be a matter of congratulation that Dr. Huggins has got to the degree of perfection he has in his researches.

Mr. Bidder: May I suggest that the diagram, to my mind, brings out very prominently the fact, which I suppose is not, however, a novelty, that lines which have a common origin may behave very differently under different conditions. Assuming that the diagram is correct, no one can doubt for a moment that the twelve lines which are shown in the spectrum of Vega, and the corresponding lines which appear in the other spectra, are attributable to one common origin, but it is prominently brought forward by the diagram how differently certain of these lines behave under the different conditions that presumably exist in the other stars. This seems to be especially the case with respect to the three lines at the most refrangible end, which entirely disappear in Sirius, and almost disappear in two others, and in the remainder are comparatively insignificant. I have only to express further a hope that the diagram may be reproduced in the *Monthly Notices.*

Mr. Common: Before Dr. Huggins replies I should like to ask one question if too many have not been already put; that is, as regards getting a certain definite width in the spectrum. Is it done by a cylindrical lens, or how is it done? because ordinarily a star would only give a linear spectrum.

Dr. Huggins: I have sent to the Royal Society a full account of the method of observing. But I will, if the meeting will allow me, answer the last question first. The method of giving width to the spectrum is not by a cylindrical lens, which for several reasons I wished to avoid; but the star has been allowed to remain upon one portion of the slit, for sufficient time to form a spectrum, and the instrument has then been moved in declination so as to bring the star a little further down upon the slit, and then a second spectrum has been obtained; and in that way you can get any breadth you wish. The photographic spectrum of the star's image is not a true line, but practically is so broad that two or three times the width is sufficient to give a spectrum broad enough for examination. With regard to Mr. Christie's question, my intention has been to represent the relative character of these lines; but this diagram does so in rather an exaggerated degree, being intended to show at a distance. In the diagram I sent in to the Royal Society these differences are given with great accuracy; the chief points being that in Vega and Sirius the first line H is about the same, I should say, as in the solar spectrum. When you come to Ursæ Majoris it will be seen that it is not so broad, and slightly narrower in α Virginis, and so on. But in Arcturus the first line H is about the width that it is in the solar spectrum, and the line K is broader, relatively to H, than in the solar spectrum. I think in the solar spectrum it varies. In Arcturus it is in a marked degree wider than K.

Mr. Christie: Then is the H line in α Aquilæ and α Cygni narrower compared with the solar spectrum than in Arcturus?

Dr. Huggins: Yes; and defined at the edge.

Mr. Christie: As shown there?

Dr. Huggins: Yes; except that it is here exaggerated. I think the next questions were those of Mr. Ranyard as to when the spectrum of Jupiter was taken. I wished to have got the slit just across the red spot. The red spot was on Jupiter; but the slit went across Jupiter just on one side of the red spot. I have not yet been able to get a photograph of the spectrum of Uranus. With reference to Mr. Neison's question, I do not see the force of his reasoning, because it is well known the difference in the spectrum is great indeed between looking at the sun at a high altitude and looking at the sun when he is low. If you look at the moon at a high altitude you have the minimum effect of the earth's atmosphere, and if the moon has a similar atmosphere, the light in going to the moon's surface and back again passes twice through the thickness of the moon's atmosphere, and would certainly suffer very considerable absorption, and though it might not produce any different kind of modification of the lines it would add to their intensity and, I think, would certainly be perceived under the circumstances; but I do not wish for a moment to say that these observations are antagonistic to a lunar atmosphere, they are merely negative evidence. I think that there was one other question.

Mr. Ranyard: Whether you have any evidence tending to show that there is any real connection between the broad H lines of the solar spectrum and the group of lines within them?

Dr. Huggins: I cannot form an opinion from my stellar work, but the probability would be, I think, that the H lines in the solar spectrum are only accidentally connected with the fine lines which happen to fall in the same part of the spectrum, and are not physically connected with them.

The President: I am sure the Fellows of the Society will accord their warm thanks to Dr. Huggins for his most interesting paper. (Applause.)

Mr. Christie read a paper *On the systematic errors of the Greenwich north polar distances from* 1836 *to* 1878. He had discussed the determination of the co-latitude from circum-polar stars, and the corrections for the position of the ecliptic as determined by north polar distance observations. The points to which attention was directed were the flexure, and the refraction. He had come to the conclusion that the determination of refractions could only be satisfactorily made by comparison with the southern observatories. There was no reason to assume any change in Bessel's refractions, which satisfied the Greenwich observations with as much accuracy as could be expected. He had taken three periods of observations; and he also went through the observations Mr. Stone had discussed from 1857 to 1865, dividing them into the same groups as he had, and he got results which did not differ, by any large quantities, from those of Mr. Stone. In some of them there was an element of uncertainty which led him to ask whether any correction was applied by Mr. Stone for the different co-latitudes assumed in the course of years, and also for the change in the law of the R—D. correction, which was different in the two periods. The results he had obtained in comparison with Mr. Stone's were as follows:—

Old R—D.	—″·19	+″·09	—″·22	—″·79	—1″·20
New R—D.	— ·21	— ·09	— ·49	— 1·11	— 1·56
Stone	— ·07	— ·03	— ·40	— ·88	— 2·02

At the end Mr. Stone got a larger residual quantity, but he (Mr. Christie) had weighted them in a slightly different way from Mr. Stone, having given equal weights to the results of every year. But if they took the old law of the R—D. correction, depending on the sine of the zenith distance, the reduction on the refraction became much less obvious. When he divided the observations into different groups there did not appear much evidence either way, as to the diminution of the refraction; but it tended, as far as it went, in favour of the old law for R—D., which neutralises the flexure correction. This showed the difficulty of dealing with corrections that could not be determined with any very great accuracy. Another determination of latitude he had discussed from observations of the sun, but here the R—D. correction did not enter into the case. Also, by way of making the results a little more accurate, he had applied corrections for the errors of the thermometer, as far as they could be determined, a point on which Mr. Marth had laid great stress, with some reason. The majority was two to one in the discussion of the observations of the sun in favour of Bessel's refractions. Then coming to the comparisons of the Greenwich observations with the observations at the Cape and at Melbourne these showed such decided discordances when the diminished refractions were used that it seemed impossible almost to admit them. Another interesting point he had discussed was the obliquity of the ecliptic. Two values of the secular variation of the obliquity had been used in the *Nautical Almanac* — *Bessel's* up to 1863, and *Le Verrier's* since. A comparison of the Greenwich series of observations from 1836 to 1878, seemed to show that Le Verrier's theoretical value was supported by the results. The figures (corrected for the error of the thermometer excepting for the first period when there was not much information) were :—

Le Verrier.	Bessel.
—″·21	+″·22
+ ·16	— ·11
+ ·20	— ·40

Another point was that this comparison afforded evidence as to the law for R—D., and it went in favour of the old law. A further point was that if they assumed Le Verrier's value, Bradley's and Piazzi's must have large errors of 1″·8 observations and 1½″ respectively. These were large quantities, but still the results seemed to show that they must be accepted as inevitable in this case.

Mr. Marth : I will only put three or four questions. The last group on the table extends to 45° north polar distance. Do you not go beyond?

Mr. Christie : I was not discussing discrepancies beyond 80° Z. D.

Mr. Marth : At the last meeting Mr. Stone referred to refractions beyond 85° zenith distance, but as his last star has less than 84° zenith distance his investigation bears in no way upon the refractions beyond 85°. The second question—there was a change made in the thermometer in 1858. I said last year that the reading of the instrument had been shifted $\frac{8}{10}$ths of a degree, and you have corrected it to $\frac{6}{10}$ths, what evidence is there for the correction?

Mr. Christie : The correction was 0°·7 in 1858 by comparisons with the standard thermometer; and I have applied 0°·6 for the period 1854-57, assuming that the error is progressive roughly from 1842.

Mr. Marth : Had the thermometer been investigated in 1842?

Mr. Christie : Not as far as I know. The thermometer was then new.

Mr. Marth : This question about the meteorological instruments was raised more than half a century ago. To get it settled the then Board of Longitude, guided by Dr. Thomas Young, presented barometers and thermometers to the chief observatories—Greenwich, Dublin, Konigsburg, Altona, and others. Whether the instruments were compared among themselves is not known. I should like to know what is the history of the Greenwich barometers and thermometers used in the circle-room from that time, 1825, to 1858. Could you not supply it? You see it is a very serious question, because it affects the Greenwich observations of that time. I mentioned last year that the difference between the mean refractions which Bessel deduced from Bradley's observations, and those which he deduced from his own observations, was reduced by taking the errors of the meteorological instruments into account, to a difference corresponding to $\frac{4}{10}$ths of a degree Fahrenheit. Will you be so kind as to make inquiries? My third question I will ask Mr. Stone and Mr. Christie: Have you read Bessel's own investigations on which the refractions in the Tabular Regio Montana are founded?

Mr. Stone: Oh, yes.

Mr. Marth: You have? Then will Mr. Stone be so good as to enlighten me upon a single figure? His corrections are given for five groups. I should like to know how any one of these corrections, say the last, which is $2''\cdot4$, or something of that kind, has been deduced from the observations. We were told at the last meeting that the verification of the whole investigation can be done in a few hours. But who is clever enough to do it? For you must take nine volumes of the Greenwich observations, and try to find out what has been done. Will you be so kind as to enlighten me?

Mr. Stone: I said the other day that the observations were corrected from the investigations for the co-latitude in the several volumes of the Greenwich observations. I had supplied small corrections, depending, as far as I could, upon the division errors that were adopted.

Mr. Marth: In your paper on nutation, in which you come to the conclusion that the co-latitudes may depend on the moon's node, you make corrections for the division errors for Polaris, though not sufficiently, but also in the paper on refraction. And I shall be obliged if you will be kind enough to inform me what other corrections beyond those for division errors you have applied.

Mr. Stone: I think I have given you the observations you asked for.

Mr. Marth (to Mr. Christie): Will you be so good as to mention at Greenwich that the observations of Polaris are still corrected for the division errors of the same lines as 20 years ago?

Mr. Christie: I think you are mistaken. A further determination of division errors was made not very many years ago.

Mr. Marth: There was a change made, but the star has moved and the observations above the pole refer to five different lines or strokes of the circle, and to observations below the pole to four strokes while the corrections only refer to one of them. I should have been glad to have pointed this out long ago, and, six years ago, I made an appeal——

The President: Possibly we are getting a little out of the record.

Mr. Ranyard: Perhaps it will be more easy to follow the discussion when these investigations have been printed.

Mr. Stone (to Mr. Marth): The question you asked me was where I got my results from? From the introduction of the several volumes of the Greenwich observations. With respect to the small question of about $\frac{2}{10}$ths of the thermometer, I do not care about it. $\frac{3}{10}$ths or $\frac{4}{10}$ths, you may take it or leave it, that has nothing whatever to do with it. We are considering a far more important question than an index error of $\frac{3}{10}$ths or $\frac{4}{10}$ths.

Mr. Marth: It is a question of $\frac{8}{10}$ths.

Mr. Stone: Perhaps I may be allowed to say one or two words with reference to the question Mr. Christie has raised. Curiously enough I had prepared a paper on this subject for this evening. As far as I understand the question we are not very much concerned with errors of even half a degree in our thermometer. A much larger question has been broached with regard to these refractions; that is, whether Bessel's refractions will carry us over a large arc of zenith distance or not. I rather think at present that part of the question has been a little shirked. Whether you take your observations above or below the pole, or take them towards the south, when you get to the angle of zenith distance of 85^{c}, Bessel's refractions will not represent the observations, and must be reduced. I take a fundamental objection to Bessel's theory, if it does not satisfy the observations; but I am by no means certain that his theory does not satisfy them. So far as my results of 1867 went, the general result was to confirm Bessel's refraction. Below 85°, the refractions required a diminution by (0·005 × refr.) roughly; and the general result of my investigation was that thep laces of the circum-polar stars both above and below pole at all less zenith distances could not well be represented unless diminished in something like the same proportion; and the consequence was the whole of Bessel's refractions checked moderately well together. Provided that result were true we should have a sound foundation for Bessel's theory, and could proceed with the utmost accuracy in the way that Mr. Marth has pointed out, but my investigation was aimed at the great principles of the theory and not at the small details. My results were substantially the same as Mr. Christie has got. His factor is about 0·004 and mine about 0·005. Curiously enough since the last paper I have been examining the circum-polar observations printed in the catalogue of 1872, in which it is stated that the circum-polar observations are not represented by these reduced refractions. In the three classes there given there is no indication of any errors of the refraction.

Mr. Christie: Is there not an error in the date, 1872?

Mr. Stone: In the introduction to your last catalogue those three groups are given. But I have taken the trouble to go through the observations of the group not included in the introduction—the groups that are lower down, the ones that Mr. Marth is inquiring about, and the residual for the group which extends down to 46°·13′ amount to 0″·72, and down to 48°·56′ it amounts to nothing more than +0·19. The correction got in the introduction is not more than $\frac{4}{10}$ths of a second, and if you apply that residual to all, the groups are reduced down to these quantities :—

+0″·14 +0″·23 —0″·04 +0″·32 —0″·21

So far as these are concerned therefore they are reconciled together by diminished refractions. Thus using Bessel's refractions with simply the diminished mean refractions and without alteration from the first degree of polar distance, down to the lowest star observed, the residuals have no reference to magnitude of the refraction factors. This is from 1857 to 1868; from 1868 to 1872. I see no reason whatever for assuming that these diminished refractions do not thoroughly represent the circum-polar observations; and if they do, considering that they extend down nearly to the horizon, undoubtedly they are entitled to confidence. I cannot follow the legitimacy of the method Mr. Christie has adopted of using the old correction for R—D. instead of the new one. The flexure should be determined very carefully by means of the opposite collimators, and having determined that, there is an end of it, and you cannot after that introduce another correction dependent on the sine.

The term $a \cos^2 Z. \sin Z.$ now introduced fully comes in in another way. Instead therefore of thinking the old form is the more legitimate, I confess that the new form of it is decidedly the more legitimate form. The numbers which apparently seem to have convinced Mr. Christie that these refractions must be wrong appear to me to confirm each other in a way that could hardly have fairly been expected. When my paper was written the only recent southern stars catalogued were by Ellery in 1863-4-5. I compared these with the Greenwich observations, they required the diminished refraction not quite to the same extent as those above and below the pole, still in the same direction to errors. As far as my own observations at the Cape are concerned they do not agree so well with the Greenwich observations with diminished refractions, and unless there are considerable systematic errors in one or the other of the instruments, the small refractions would be inadmissible.

The President: Is it your pleasure to record your thanks to Mr. Christie for his paper? (Applause.)

Mr. Dunkin read a paper in reply to the paper by Mr. Neison, published in the last *Monthly Notices,* in regard to personality in the Greenwich tabular errors.

Mr. Neison said he would furnish a short paper in refutation of Mr. Dunkin's views.

Mr. Marth made an appeal to members to observe the brightness of Mars towards the end of February, when it would be conveniently situated for comparison with respect to α Orionis and α Tauri.

The President announced a communication from Dr. Wolf, of Zurich, asking for biographical information with regard to the Rev. John Mitchell, Nathaniel Pigott, Edward Pigott, Sir Henry Charles Englefield, John Goodriche, Thomas Wright, of Durham, and Robert Wright.

Captain Noble drew attention to the long time occupied by one or two papers to the total exclusion of others of general interest,* and suggested that mathematical papers were better read than orally explained.

Mr. Ranyard reminded Captain Noble that the discussion had been a very important one, and would be interesting to foreign astronomers.

The following papers were not read owing to want of time:

C. Todd: *Observations of the phenomena of Jupiter's satellites, &c., at the Observatory, Adelaide.*

W. F. Denning: *Meteor showers.*

Astronomer-Royal: *Observations of occultations of stars by the*

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*[Note by Captain Noble.] In justification of my complaint I invite especial attention to the fact, that (with the exception of Mr. Stone and Mr. Marth) every Fellow present was either reading, talking, or sleeping during the time wasted by Mr. Christie; who practically occupied half of the night in chalking co efficients on the blackboard, and in giving the dreariest and most monotonous details of corrections.

moon, and phenomena of Jupiter's satellites, made at the Royal Observatory, Greenwich, in the year 1879.

H. Corder: *Meteor showers,* 1870-79.

C. P. Pritchard: *Note on the re-measurement of a lunar photograph, in reply to Mr. Neison's criticism.*

C. E. Burton: *Changes of relative brightness of Jupiter's satellites,*

H. Sadler: *Notes on "A catalogue of* 10,300 *multiple and double stars," &c., forming Vol. XL. of the "Memoirs of the Royal Astronomical Society," Hours,* 0—*VI.*

T. W. Backhouse: *Estimations of the time of transits of the red mark on Jupiter across the central meridian.*

Astronomer-Royal: *Mean area of spots and faculæ upon the sun's disk, as measured on photographs for* 1878 *and* 1879.

Astronomer-Royal: *Observations of the outer satellites of Mars made at the Royal Observatory, Greenwich.*

A. M. W. Downing: *The Greenwich standard, right ascensions.*

N. E. Green: *On some changes in the markings of Mars.*

The meeting adjourned at ten o'clock.

THE LICK OBSERVATORY.

In the *San Francisco Chronicle,* October 18th, there is a long article on the Lick Observatory. It will be remembered that by the princely munificence of the late James Lick, who died two years ago, the sum of 700,000 dollars was devoted to the purchase of the site for and the construction of a telescope with mounting and appliances, that should be more powerful than any yet made. The visit of Professor S. W. Burnham has determined that Mount Hamilton, in Santa Clara County, will be the future site of the finest astronomical and meteorological station in the world. Any surplus of the above sum remaining is to form an income for the maintenance of the observatory. Already a road 20½ miles long, from Santa Clara to Mount Hamilton, has been finished, and a large portion of land for the observatory and other buildings purchased. After a residence of above a month, and examining a great number of test objects with his 6-inch Alvan-Clark refractor, Professor Burnham was confirmed in his opinion that Mount Hamilton was one of the best astronomical sites in the United States. It has a fine broad sweep of horizon, and a sky ordinarily cloudless all the summer, and free from fogs and other hindrances. This opinion is shared by Professor Newcomb and by Professor Soule, of the State University. A glass of the projected size and quality will require fully three years in construction. The exact size has not been determined, and will depend somewhat upon the success of the Struve glass which Alvan Clark and Son have recently contracted to furnish to the Imperial Observatory in St. Petersburgh, of 30-inch diameter, at a cost of 32,000 dollars. But with a view to the observation of the transit of Venus in 1882, the trustees propose to secure at once a 12-inch refractor, which will become a permanent fixture of the observatory. Mount Hamilton seems to be considerably above 4,300 feet high. The formation of its summit is trap rock and porphyry, with croppings of metamorphic slate lifted by later upheavals. The main observatory building will be about 70 feet in diameter. The foundation will be stone and brick, the walls iron and steel. In addition to the large telescope and its smaller companions, there will be an equatorial, a meridian circle, and other supplemental instruments. Professor Burnham has also proved the excellence of the location as a future meteorological station. We heartily rejoice at the active prosecution of this great work. If unsparing expense, and the command of first-rate optical and mechanical skill shall eventually produce an objective of between 30 and 36 inches, peerless in quality as in size, it will be most gratifying, not only to the astronomers of America, but of all countries, that on a site peculiarly favoured by nature, an unrivalled instrument is giving daily effect to the generosity and noble scope of the founder, and advancing our knowledge of the heavens.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

OUR ANNUAL MEETING—AN APPEAL TO THE FELLOWS.

Sir,—In common, I presume, with many other Fellows of the Royal Astronomical Society, I have just received an anonymous circular, calling on us to remove our old servant, Mr. Ranyard, from his post of senior Secretary, in order to substitute in his place Mr. W. H. M. Christie, of Greenwich. The only reason alleged for making such a change is the statement that it is the opinion of some Fellows that the status and interests of our Society have suffered by neither of the Secretaries being practical astronomers.

I have read this circular with regret and surprise, for though I am not astonished to find that some of our Fellows are dissatisfied with some of the steps taken by Mr. Ranyard, in his zealous labours to advance the interests of our science and Society, I did not believe that they would have challenged his labours on other than the real ground of their dissatisfaction, and have endeavoured to have displaced him from his present position on fictitious pretences.

To myself, as to most of us, it is well known that some of our Fellows are dissatisfied with Mr. Ranyard, but I do know that the reason for this dissatisfaction is not that alleged in this anonymous circular. Had it merely been the desire to have a "practical" astronomer as one of the Secretaries, then in compliance with common courtesy it is Mr. Glaisher who should retire as the junior Secretary. I do not hesitate to affirm that it is Mr. Ranyard personally who is disliked, and that the change is directed against him personally.

Though one of the younger Fellows of the Society, I have had sufficient experience to know that the reason alleged in this circular for displacing Mr. Ranyard in favour of Mr. Christie is utterly fallacious. The duties appertaining to the post of Secretary are not such as to render it essential that he should be a "practical" astronomer, by which term seems to be meant a mere routine observer in some public observatory. If any special knowledge of the detail of observatory work be wanted that knowledge can be supplied as in other cases by any competent ordinary member of Council. If a paper is to be read which might seem to require some special knowledge of this kind, then, as in other cases, there is always one or more ordinary member of Council who is both competent and willing to read the paper. The important duties of the Secretaries are to carry out the business of the Society according to the directions of the Council, to be on the alert to further the interests of the Fellows in the Society by every available means, to promote astronomy by enabling the Fellows to gain any information they may require, to be of assistance to our Fellows at the evening meetings, and in short in all proper ways to promote the interests and welfare of our science and Society by facilitating the scientific labours of our Fellows. How far would the possession of some abstract technical knowledge of the kind displayed at the last meeting aid them in these labours?

It is very desirable that we should frankly deal with one another. I do not hesitate, therefore, to affirm that the real reason why a number of Fellows desire to replace Mr. Ranyard by Mr. Christie is entirely different from that stated in the anonymous circular, and that it has its origin in the party differences which have caused so much ill-feeling in the Society. Who issued this circular I do not know, I do not much care, but it is certain it would not have appeared without Mr. Christie's authority. Whatever intention it may have been issued with I do not know, but I do know that in its effects it might just as well have been issued as a party manœuvre.

As previously thereto, since I have been honoured by a seat on the Council, I have refrained from identifying myself with any party on the Council. I may have seemed to have tended more in one direction than another, but the cause is very simple. Like so many of our Fellows, I am a simple amateur, and I recognise, therefore, that we have many wants which professional astronomers do not always comprehend, and seldom sympathise with. I have endeavoured to make it understood that we have wants, aims, ideas, and interests which though trivial in their eyes are not necessarily so in ours, and thus much which seems highly important to them may possess less vivid interest to us. In the furtherance of these objects I have had more assistance from Lord Lindsay and Mr. Ranyard than from those of my colleagues who are professional astronomers, I have naturally seemed more in union with them than with others. I claim, however, to be a perfectly independent Fellow of the Society, like most of my fellow amateurs.

Now, it is useless to attempt to disguise the matter, The issue of this circular is to all intents and purposes a manœuvre in the interests of those of our Fellows who are personally dissatisfied with their present position in regard to the management of the Society. They wish to effect a complete revolution in the Council, so as to secure the management in their own hands, so as to enable them to check its present progress, and direct it into another path. This attempt to displace Mr. Ranyard is merely the thin edge of the wedge. If successful, at the very next opportunity it would be followed up by the removal of a number of the other members of the Council. And though I give Mr. Christie and his party the credit of having no present intention of pushing matters to such an extreme, experience shows that the mere force of events would end in their excluding from the Council all who did not belong to a clique of official astronomers, except the few, like Lord Lindsay, Prof. Cayley, and Mr. Lassell, whose great eminence rendered this exclusion impossible.

Personally I should be very glad to see Mr. Christie on the Council. As many Fellows well know, I have been canvassing for him. Even now I have devised a means, which I hope will be adopted at the annual meeting, and which will provide Mr. Christie with a seat on the Council, without hurting anyone's feelings, or really weakening the interests of the Society in a manner which must ensue if the change proposed in this anonymous circular be carried out. But as Secretary is another thing. Mr. Christie and Mr. Ranyard must be known to most of our Fellows, and they can form their own judgment as to who would form the best Secretary, but I believe that in any case a change in the manner suggested would be most injurious to the interests of the Society.

I would also point out to the Fellows that the real character of the step they are asked to take is most mischievously disguised in this circular, and that the step there suggested would inevitably lead to Mr. Ranyard's complete exclusion from the Council. It says, displace Mr. Ranyard in favour of Mr. Christie, but put Mr. Ranyard's name on the list as an ordinary member of Council. But if so, in place of whom? If Mr. Ranyard is to go on the Council as an ordinary member, who are you going to displace to make room for him? Who displace? Unless all fix on the same person Mr. Ranyard must fail to be elected. Is it fair or candid to suggest that the proposed change only means shifting Mr. Ranyard from the Secretaryship to a post of ordinary member of Council, when it really means that either he or some other member is to be entirely excluded?

For this reason, therefore, I appeal to our Fellows, though a simple amateur astronomer like most of themselves. I confidently appeal to them to support me in the view I have urged, and to refuse to make the change suggested, and though I know I appeal to many who are older, wiser, and better astronomers than myself, I do so on the ground that my neutral position has led me to be better acquainted with this party manœuvre than many of those who may have had less opportunity of knowing the true character of this anonymous circular.

E. NEISON.

LUNAR CORONA.

Sir,—My December full moon experiments have given me results similar to those described by Mr. A. C. Ranyard in your January number, and I also at that time obtained the same results from artificial light, but instead of the halo (or rather ring) being produced by reflection only, I think that there is first refraction and then reflection. Hitherto I have been unable to obtain lunar rings on collodion-coated plates, but I do not doubt if I had clockwork arrangement attached to the camera, so as to command a steady and prolonged exposure, that I could obtain rings and half-rings at other phases of lunation.

The delicacy and accuracy of delineation which this new process of photography affords to astronomical research will, I have no doubt, provide a mode of measuring the intensity of the light of celestial bodies as well as calculating their distances. With your permission I will give the experiment in detail.—I took two extra sensitive brown-gelatinized plates of *different thickness of glass* and exposed the image of the moon at full moon phase three times on each plate, giving sixty seconds to each exposure one after the other in quick succession, and developed them both in one solution, treating them in all respects the same. On the thin plate were three rings of one size, small in circumference, dense and well-defined, while the thick plate gave rings of larger diameter, less dense, but well-defined, and like the smaller rings, uniform in size. The difference in size of the rings on the two plates—small rings on the thin plate and rings of larger circumference on the thicker plate—proves that the rays are reflected from the back surface of the plate *only*. I say only, because unsized light absorbing paper was carefully placed on the back part of the plate in order to prevent any reflection except that from its inner surface, although I think that a mirrored back would illustrate the experiment better. The ringed appearance produced on brown-gelatine plates by strong light is not what is usually obtained on collodion-coated plates from over exposure, called by photographers solarization, because there we have no distinct ring, no marked indication of refraction, but mearly burr radiating in all directions from the luminous point, whereas with gelatine plates there is a clearness of definition between the impinging and reflected light, and when the experiment is carefully conducted ring within ring may be obtained, showing that other rays than the indigo and violet rays are reflected and also recorded. No doubt this experiment explains the cause of the rings, but it does not account for the great increase of actinic force manifested at full moon period. Up to fourteen hours before full moon I fail to get rings, but at full moon and for thirty-six hours after I get them easily with sixty seconds exposure. There is a point at which self-luminous and reflected light registers its refracted rays, and from the simplicity and accuracy of the experiment I think a standard of comparison may be established by which light may be measured.

If a circular hole, say one inch in diameter, be made in the side of a dark chamber and the light to be tested, solar, lunar, electric, coal gas, or wax, placed at a known distance from the aperture outside, and a camera of known focal length placed inside the dark chamber in a line with the aperture and the light outside, and the image of the illuminated disk focussed to small compass, say $\frac{1}{16}$ of an inch in diameter, then a series of impressions half an inch apart may be taken on a plate coated with bromo-gelatine emulsion of known composition and glass of certain thickness, the time of each exposure ranging 1, 3, 5, 10, and every additional 5 seconds on to 60, or 360, or longer, and develop the images with a solution of known strength. Note the ring which appears first on the line of impressions and connecting it with the time of its exposure, it may be adopted as a standard of comparison from which the intensity of other lights may be calculated.

GEORGE BERWICK.

Sunderland: Jan. 10, 1880.

NOVA CYGNI AND NEIGHBOURING STARS.

Sir,—It would be interesting to hear whether this star is still visible in powerful telescopes, as it is so no longer in my 4¼-inch refractor. The nights, however, were not quite perfect when I last looked for it. Is not this continued fading opposed to the idea of its being a planetary nebula? for it is certainly now much fainter than the 12½ mag. star noted by Schmidt as following it by 4·6s. and 20″ further N. I have never been able to see with certainty Schmidt's 13 mag. star lying 45″ S. of the Nova, and 1s. preceding, though I have seen his 13 mag. one lying in the same direction from the Nova, but at double the distance, and there is a still brighter star a little past this, not mentioned by Schmidt.

I should like to ask J. E. Gore whether he has ever made out anything further about the stars he mentioned in your vol. for 1877, p. 124, as possibly variable. The star which he thought he caught glimpses of with his binocular, lying about 23′ s.p. the Nova, is not visible at all with my telescope, and I do not think it was so in January, 1877, either, as I have not entered such a star in my map of the region. The star Lalande 42360 exists, but instead of being of the 7th mag. it is quite small.

Yours truly,

Sunderland: Jan. 8, 1880. T. W. BACKHOUSE.

ASTRONOMICAL OCCURRENCES FOR FEBRUARY, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars.
Sun	1	17 28	Near approach of 67 Virginis (5½) Sidereal Time at Mean Noon 20h. 44m. 19·83s.	1st Sh. I. 3rd Tr. E. 1st Sh. E. 3rd Oc. D.	4 52 6 25 7 9 7 26	— 6 40·9
Mon	2		Sun's Meridian Passage 13m. 54·92s. after Mean Noon			6 38·6
Tues	3	3 38	☾ Moon's Last Quarter			6 36·4
Wed	4	16 4	Occultation reappearance of σ Scorpii (3½) Saturn's Ring: Major axis=37″·90 Minor axis= 5″·78			6 34·2
Thur	5	18 11 19 23	Occultation of B.A.C. 5909 (6½) Reappearance of ditto			6 32·1
Fri	6					6 29·9
Sat	7	0	Conjunction of Moon and Venus 14° 7′ N.	2nd Ec. R.	7 29 42	6 27·8
Sun	8			1st Tr. I. 1st Sh. I.	6 9 6 48	6 25·7
Mon	9	23 17 20	● New Moon Conjunction of Moon and Mercury 5° 15′ S.	1st Ec. R.	6 20 17	6 23·6
Tues	10					6 21·5
Wed	11					6 19·5
Thur	12	5 27 6 32 5 32 6 26 3	Occultation of 9 Piscium (6) Reappearance of ditto Occultation of κ Piscium (4½) Reappearance of ditto Conjunction of Moon and Jupiter 6° 16′ S.	3rd Tr. E	5 46	6 17·4
Fri	13		Sidereal Time at Mean Noon 21h. 31m. 38·50s			6 15·4
Sat	14	2 7	Conjunction of Moon and Saturn 8° 7′ S. Superior conjunction of Mercury and Sun Illuminated portion of disk of Venus=0·780 Illuminated portion of disk of Mars=0·894	2nd Oc. D.	6 16	6 13·4
Sun	15		Sun's Meridian Passage 14m. 23·66s. after Mean Noon			6 11·5

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Mon	16	7 37	Occultation of μ Arietis (5½)	1st Oc. D.	5 31	Mars. —
		8 44	Reappearance of ditto			6 9·5
Tues	17	15 45	☽ Moon's First Quarter			Sirius.
		5 10	Occultation of 66 Arietis (6½)			
		5 58	Reappearance of ditto	1st Sh. E.	5 29	—
		10 40	Near approach of 9 Tauri (6)			8 51·0
		22	Conjunction of Moon and Mars 1° 37′ S.			
Wed	18	7 16	Occultation of 62 Tauri (6)			8 47·1
		8 25	Rappearancee of ditto			
Thur	19					8 43·2
Fri	20	8 17	Occultation of 5 Geminorum (6)	Jupiter's satellites invisible through nearness to the Sun.		8 39·2
		9 9	Reappearance of ditto			
Sat	21					8 35·3
Sun	22	13 44	Near approach of B.A.C. 2683 (6)			8 31·4
		17 8	Occultation of ζ^1 Cancri (4½)			
Mon	23	0	Mars at quadrature with the Sun			8 27·4
Tues	24	5 18	Occultation of ξ Leonis. (6)			8 23·5
		5 32	Reappearanec of ditto			
		10 39	Occultation of o Leonis (3½)			
		11 54	Reappearance of ditto			
			Saturn's Ring; Major axis=36″·99 Minor axis= 6″·22			
Wed	25	13 21	○ Full Moon			8 19·6
		15	Opposition of Uranus to the Sun			
Thur	26	17 6	Occultation of e Leonis (5)			8 15·6
		18 6	Reappearance of ditto			
Fri	27	16 52	Near approach of 14 Virginis (6½)			8 11·7
Sat	28	13	Conjunction of Jupiter and Mercury 0° 40′ N.			8 7·8
Sun	29					8 3·8
MAR. Mon	1					7 59·9

THE PLANETS FOR FEBRUARY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	20 28 32	S. 21 3	4″·8	23 40·3
	9th	21 24 3	S. 17 28	4″·8	0 1·2
	17th	22 12 58	S. 13 2	4″·9	0 25·5
	25th	23 8 22	S. 6 39	5″·3	0 49·3
Venus ...	1st	18 13 54	S. 21 54	14″·9	21 26·0
	9th	18 55 27	S. 21 46	14″·3	21 36·0
	17th	19 36 55	S. 20 58	13″·7	21 45·9
	25th	20 17 55	S. 19 30½	13″·1	21 55·3
Mars ...	1st	3 26 17	N. 20 45½	10″·7	6 40·9
	9th	3 40 31	N. 21 34	9″·9	6 29·9
	17th	3 55 58	N. 22 21	9″·3	6 7·6
	25th	4 12 26	N. 23 4½	8″·7	5 52·5
Uranus ...	2nd	10 39 37	N. 9 21	4″·2	13 49·1
	18th	10 37 9	N. 9 36	4″·4	12 43·7
Neptune ...	2nd	2 29 60	N. 12 55	...	5 40·8
	18th	2 30 45	N. 12 60	...	4 38·6

Mercury is badly situated for observation till the end of the month.

Venus rises about three hours and a quarter before the sun on the 1st, the interval decreasing.

Mars sets on the 1st about two hours and three quarters after midnight, the interval decreasing.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN FEBRUARY, 1880.

BY W. R. BIRT, F.R.A.S., P.S.S.

From the 14th to the end of the month the following objects may be observed:—Cinsoninus; a very bright crater of 9°. A comparison of its brightness with those of Dionysius and Proclus also rated at 9° will be valuable. Godin and Agrippa, two conspicuous craters, well shown on Common's photogram of April 21, 1877, on which is seen very distinctly a ring not on Neison's map. It is situated just N.E. of a line joining Godin and Rhæticus; the scale of the photogram, 3 inches, is too small to bring out the smaller craters, and it is likely that this apparent ring may really result from a mountain which appears upon one of De La Rue's photograms. On a diagram of the Selenographical Society, 1880, No. XII., this ring is marked *a*, and has two craterlets on it marked respectively 11 and 12; drawings of this ring and the craterlets in its neighbourhood are very desirable. Copernicus, the Riphæan Mountains, Encke, Kepler, and Mersinus are interesting objects; careful drawings of them would be acceptable. After full the surface of the Mare Crisium may be well observed, particularly the ridges and minute craterlets occurring on it.

Fracastorius.—We find from a letter in the *Selenographical Journal*, January 11, 1880, that Mr. David Simms in 1876 carefully observed the ridge extending from the extremity of the east wall to a shallow crater or partially submerged ring; this ridge is said to have three peaks on it. The verification of the existence of these peaks is necessary for the purpose of completing the synopsis of objects in and near the great crater.

Water Lane, Stratford, E.:
January 20, 1880.

SATELLITES OF URANUS.

Major and minor semi-axes *a* and *b* of the apparent ellipses described by the satellites, and position-angles p_0 of the major axes:

	Ariel.		Umbriel.		Titania.		Oberon.		
1880.	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	p_0
	″	″	″	″	″	″	″	″	°
Feb. 4	15·21	2·07	21·19	2·89	34·76	4·74	46·49	6·33	13·91
14	15·26	2·18	21·26	3·04	34·87	4·99	46·63	6·67	13·84
24	15·28	2·30	21·29	3·20	34·92	5·25	46·69	7·03	13·76

In these apparent orbits the satellites move in the direction of decreasing position-angles, from their greatest northern elongation N. (in position angle 14°) to inferior conjunction (posit. 284°), greatest southern elongation S. (pos. 194°), superior conjunction (pos. 104°), etc. The satellites will be at these points of their orbits about the following hours, G.M.T.:

	Ariel.			Umbriel.			Titania.			Oberon.	
1880.	h.			h.			h.			h.	
Feb. 1	18	N.	Feb. 1	16	N.	Feb. 1	22·7	sup.	Jan. 31	8·7	S.
3	0	S.	3	18	S.	4	2·9	N.	Feb. 3	17·5	sup.
4	6	N.	5	19	N.	6	7·2	inf.	7	2·3	N.
5	12	S.	7	21	S.	8	11·4	S.	10	11·0	iuf.
6	19	N.	9	23	N.	10	15·7	sup.	13	19·8	S.
8	1	S.	12	1	S.	12	19·9	N.	17	4·6	sup.
9	7	N.	14	2	N.	15	0·1	inf.	20	13·4	N.
10	13	S.	16	4	S.	17	4·4	S.	23	22·2	inf.
11	20	N.	18	6	N.	19	8·6	sup.	27	7·1	S.
13	2	S.	20	8	S.	21	12·9	N.	Mar. 1	15·9	sup.
14	8	N.	22	9	N.	23	17·1	inf.	Ariel.		
15	14	S.	24	11	S.	25	21·3	S.	Feb. 24	10	N.
16	21	N.	26	13	N.	28	1·6	sup.	25	16	S.
18	3	S.	28	15	S.	Mar. 1	5·8	N.	26	23	N.
19	9	N.	March 1	16	N.				28	5	S.
20	15	S.							29	11	N.
21	22	N.							Mar. 1	17	S.
23	4	S.									

The apparent orbits are now so contracted, that the question of the relative brightness of the satellites, and that of the distances from the planet at which they become invisible, may be practically settled.

A. M.

THE MINOR PLANETS, CERES AND JUNO.

The minor planet Ceres, the first of the series, comes into opposition on the 12th instant.

	Rt. Ascen.			Declin.			
	h.	m.	s.		°	′	″
9th.	10	10	10·21.	N.	27	39	15.
12th.	10	7	31·29.	N.	28	0	51.

Juno, the third of the series, comes into opposition on the 24th inst.

	h.	m.	s.		°	′	″
22nd.	10	23	38·28.	N.	3	59	17.
24th	10	21	58·83.	N.	4	19	29.

Books received.—Note sur la tache rouge observée sur la planète Jupiter. Par L. Niesten. Bruxelles: F. Hayez. 1879.—Coincidence of the bright lines of oxygen in the solar spectrum. By Henry Draper.—Photographing the spectra of the stars and planets. By Henry Draper.—The solar spectrum in 1877-78. By Piazzi Smyth. Edinburgh. 1879.—Sun, moon, and stars. By A. Giberne. London: Seely, Jackson, and Halliday. 1880.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1879.

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To March, 1880.

Lewis, H. K.

To Dec., 1880.

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All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

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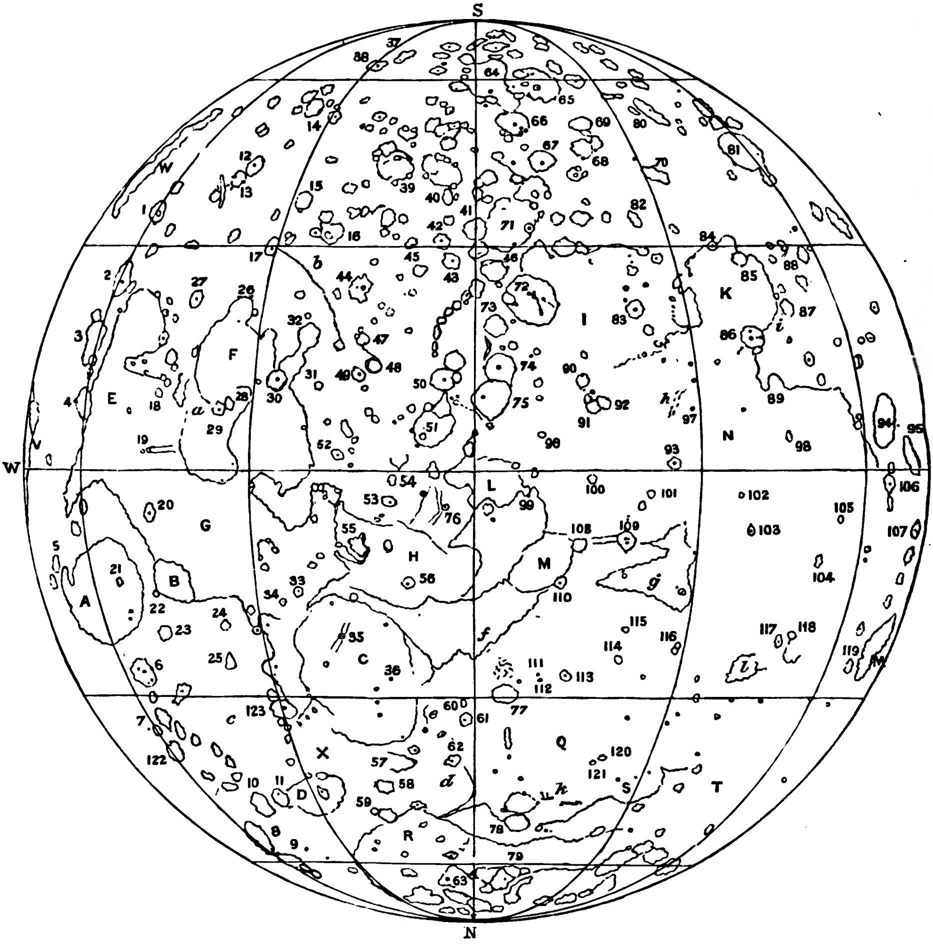
The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**



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The Astronomical Register.

No. 207. MARCH. 1880.

ROYAL ASTRONOMICAL SOCIETY.

Session 1879—80.

The Annual General Meeting of the Society was held at Burlington House, on Friday, February the 13th, 1880.

Lord Lindsay, M.P., F.R.S., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

Walter H. Bartlett, Esq., 100, Abbey Road, Kilburn, N.W., and
Captain John Steele, Marine Board, Tower Hill, E.
were elected Fellows of the Society.

The minutes of the last annual meeting were read and confirmed.

The Auditors' report was submitted by Mr. Campbell, and ordered to be appended to the annual report of the Council.

The President said: Before the annual report is read I should like to mention to the Fellows that the Eclipse Volume (Volume XLI. of the *Memoirs*) is now ready for distribution. It has occupied Mr. Ranyard for the last eight years and a half, during which time he has devoted all his leisure time to it. (Applause.) The Astronomer-Royal is not here to speak on the subject, but if he were here I think he would have had something to say about it. Only twenty-five copies have been sent in, so that there will not be enough for all the Fellows present. Two hundred copies were expected, but the binder has failed to deliver them. But I understand that more are expected to-morrow.

Mr. Ranyard then commenced reading the report. It stated that 31 Fellows and four Associates had been elected during the past year. Twenty-four Fellows and one Associate had died in the course of the year, and the total number of Fellows and Associates of the Society was now 635.

£1,074 had been invested in the course of the year, and the income of the Society, including money received from the sale of publications, interest on investments, and £700 received for the purchase of the advowsons of Hartwell and Stone, amounted to £2,742.

With respect to the instruments of the Society, Mr. Ranyard mentioned that an instrument which had been catalogued in the old lists of the Society as Abraham Sharp's quadrant, and which had been missing for some years, had been found in the course of the year. It was accompanied by some notes in the handwriting of Dr. Priestly, which described it as "Abraham Sharp's *Quadrate*." Considerable interest attaches to this instrument, as Abraham Sharp constructed and graduated a great mural sector for Flamsteed at the Royal Observatory, Greenwich, and was considered to have brought the hand-graduation of instruments to a much higher degree of perfection than had up to that time been attained. The Read Transit has also been found, and now all the instruments belonging to the Society, with the exception of a thermometer, have been accounted for.

In the list of Fellows of the Society who have died during the past year is the name of the Very Rev. Dean Hamilton, who was elected in 1820, the first year of the Society's existence. The report also contains lives of Sir Rowland Hill, the Rev. Cooper Key, Professor Clifford, Mr. Drach, and Professor Lamont.

Eighteen minor planets have been discovered during the year, and their number now amounts to 211.

Five comets have been under observation since the last annual meeting. Three of them are believed to be new comets. One was discovered by Mr. Lewis Swift, one by M. Palisa, at Pola, and a third by Dr. Hartwig, at Strasburg. The remaining two are periodic comets, viz., Brorsen's comet of short period, and Tempel's periodic comet of 1867.

The report also contained accounts of the work accomplished at the chief English and Colonial observatories, and notices of the principal astronomical publications and investigations that had appeared in the course of the year.

The President said: With reference to the comets mentioned in the report, I may mention that a telegram has been received from Dr. Gould stating that he has just observed a great comet passing northward. I should have sent out a circular about it if the news had not already been printed, and I did not feel myself justified in reprinting the announcement without leave. As soon as the comet is observed here I hope that news of it will be forwarded to me, and I will use my best endeavours to forward the information to the Society. (Applause.)

Mr. Dunkin: Which way was the comet going?

The President: Northwards.

Mr. Dunkin: We have received two telegrams at Greenwich, one stating that it was going northwards, and the other a day later stating that it was going southwards. So that I fear we shall not see it in England.

The President: I am sorry to hear that. The only news I received was, as far as I remember, "Great comet passing northward." I have to announce that no medal has been awarded by the Council this year, and I have now to ask some Fellow to propose, and another to second, "That the report be received and adopted, and that it be printed and circulated as usual."

Mr. Harley: I have much pleasure in proposing, "That the report of the Council now read be received and adopted, and that it be printed and circulated in the usual manner." It is not necessary to add any word to this resolution.

Mr. Downing: I beg to second the resolution.

The resolution was put and carried unanimously.

The President then appointed Mr. Beck, Mr. Talmage, and Mr. Rand Capron to act as scrutineers of the ballot for the election of officers and Council.

Mr. Mattieu Williams: I should like to ask a question with reference to the re-election of officers. I received the other day a balloting paper in which it is proposed to substitute the name of Mr. Christie for that of Mr. Ranyard as Secretary of the Society. The question I wish to ask is, whether Mr. Christie himself is desirous of this alteration?

Mr. Freeman: I think this question can scarcely be put with any advantage to the interests of the Society, because it is a question which could not very well be answered (Laughter.) In point of fact, if the Society decides to confer the honour on Mr. Christie, Mr. Christie will doubtless favour the Society by accepting it. (Laughter.)

Professor Pritchard: If Mr. Christie, or any other gentleman, is really desirous to have such a place, I for one do not think he is the person for the place. (Applause.) It is so important an office that I think it should be rather thrust upon a gentleman on account of his fitness than that he should search and sue for it. (Applause.) And, gentlemen, I think the observation I have presumed to make should apply not merely to the Secretaryship but to all the offices of the Society. They are not offices that ought to be *desired.* No man who has a character to make should be the person selected to assist in governing the Society, but only persons who have made a character (applause), and to whom, on account of their character, we can entrust without fear the government of what ought to be a noble Society. (Applause.)

Sir Edmund Beckett: Mr. Christie resigned his place voluntarily on the Council last year, and declined to be put up again. I think it right to give that information in answer to the gentleman who asked the question.

Mr. Christie: As a remark has been made about my own personal conduct, perhaps I may be permitted to explain the motive which led me to resign my seat on the Council, for it seems to be misunderstood. I have always urged that there should be every year a considerable infusion of new blood in the Council, and as it seemed invidious to suggest that anybody else should go off. I thought I would set a good example and go off myself. (Applause.) I never intended to withdraw from the Society or to refuse to serve. (Applause.) But I put myself in the hands of the Society. I do not wish to put myself forward for any place on the Council. Of course there is considerable trouble and labour in attending to these matters, and I can see round me here and in the list of Fellows a great many who would be more fitted than I should be. (Applause.) Therefore, I do not wish to put myself forward in any way. But I did not resign my seat on the Council, as Sir Edmund Beckett has put it, saying I would not serve again. I simply retired for a period, in order to give some slight impulse to the principle which I thought it well for us to adopt. (Applause.)

Mr. Neison: Mr. Christie remarks that he does not wish to put himself forward as a member of the Council, and I quite understand that at the same time that does not apply to any other gentleman in the room who does wish to see Mr. Christie on the Council, and who would be glad to urge his claims on the meeting, Now, I have always been desirous of seeing Mr. Christie on the Council, and I think it would be well if he were elected.

A Fellow: I rise to order. Is it right and proper that there should be a discussion on the merits of the candidates? (Applause.)

The President: On the question of order, I believe there can be no doubt that Mr. Neison is not out of order, but it rests entirely with the Society to say whether they will have a discussion of this description or not. There is no business before the Society, a simple question has been asked, and the discussion is on the question.

A Fellow: I rise on the same point. It may not be, perhaps, out of order, but is it a matter of good taste? (Applause.)

Mr. Neison: Perhaps the meeting will allow me to continue. (Cries of "No, no," and interruption.)

Mr. Harley: I rise on a point of order. I think it is quite competent for the meeting to pass a resolution as to whether the question be discussed or not, and I move, "That simple questions be addressed and answered, and that no discussion take place," (Applause.)

Dr. Pinches: I second that proposition, because I see that this discussion would be interminable. If we are to discuss the qualifications of every officer to be elected, we should have to adjourn *sine die*. (Applause.)

Mr. Neison: May I ask the Society to listen to me. (Cries of "No, no," and interruption.) I am not proposing to say anything personal with respect to Mr. Christie. (Renewed interruption.)

A Fellow: I rise to a point of order. There has been a motion proposed and seconded. I want to know if Mr. Neison is speaking to the motion that has been proposed and seconded? If not, and nothing has been said, I presume the motion should be put to the meeting before Mr. Neison continues any remarks he may have to make. (Applause.)

The President: I should have put the motion to the meeting before, had I not gathered from Mr. Neison that he had a remark to make which he asked the Society to hear before the resolution was put. It is for the Society to say whether they will hear his remarks before the resolution is put.

Mr. Neison: I wish to speak—— (Cries of "No, no.")

The President: You can only speak on the motion.

Mr. Neison: I thought I was speaking on the motion.

The President: I had better now put the motion.

The motion was then put and carried.

Mr. Chambers: I wish to ask a question. About twelve months ago the Society had laid before it a large number of very useful draft alterations in the Bye-laws, but for some reason or other, which I forget now, these alterations were withdrawn, and I should like to ask whether it is proposed by the Council to reopen the question of improving the Bye-laws, and if not what would be the proper form of action for a private Fellow to take? I ask this question because I believe a large number of Fellows are rather dissatisfied with our present election arrangements, and desire to introduce some system of voting by proxy. If the Council are not prepared to invite some change, I propose to bring forward a motion on the subject, if the Council will put me in the way of doing so.

The President: The method of electing the officers and Council, and also the method of bringing about any change in the Bye-laws of the Society, are already laid down in the existing Bye-laws. So far as I recollect, it is necessary for five Fellows to sign a requisition, which is to be sent in to the Council asking them to convene a special general meeting, and stating the object for which the meeting is to be convened. No other business can be transacted at the meeting, except that stated in the notice paper. If the Council do not think fit to convene such a general meeting, I believe the Fellows of the Society who signed the requisition may convene a meeting themselves.

Mr. Chambers asked whether the Council intended to bring forward any modification in the present Bye-laws?

Lord Lindsay: That matter was thoroughly discussed, I think about two years ago, and at that time it was considered desirable to drop the question. Personally, however, I should prefer to see considerable changes in them. (Applause.) I think the Bye-laws are antiquated and the language obscure. I am not aware, however, that the Council has at the present moment any intention of introducing such a question.

Mr. Brett: You did not say what the proper method was for a private member to move in the matter.

The President: I think that was the first thing I said—that a private member could send in a requisition to the Council, signed by himself and four others, requesting the Council to convene a special general meeting.

Mr. Neison: May I ask if I am not at liberty to make any remarks whatsoever on balloting matters? Will the Society not allow me to say one word of explanation?

Sir Edmund Beckett: I may just suggest to Mr. Neison that one word always leads to two. (Laughter.)

Mr. Neison: This word will not lead to another word. It is simply to request the meeting to leave my name out of the balloting sheet, and to insert Mr. Christie's instead. (Cries of "Oh, Oh.")

The President: As there is no further business before the meeting, we will now proceed to the ballot for the election of the officers and Council.

On the return of the scrutineers, it was announced that the following gentlemen had been elected:

President.

J. R. Hind, Esq., F.R.S., Superintendent of the Nautical Almanac.

Vice-Presidents.

J. C. Adams, Esq., M.A., LL.D., F.R.S., Lowndean
Professor of Astronomy, Cambridge.

Sir G. B. Airy, K.C.B., M.A., LL.D., D.C.L,, F.R.S., Astronomer-Royal.
Arthur Cayley, Esq., M.A., LL.D., F.R.S., Sadlerian Professor of Pure Mathematics, Cambridge.
Edwin Dunkin, Esq., F.R.S.

Treasurer.

Francis Barrow, Esq., M.A.

Secretaries.

J. W. L. Glaisher, Esq., M.A., F.R.S.
W. H. M. Christie, Esq., M.A.

Foreign Secretary.

Lord Lindsay, M.P., F.R.S.

Council.

Sir Edmund Beckett, Bart., M.A., LL.D., Q.C.
A. A. Common, Esq.
William Huggins, Esq., LL.D., D.C.L., F.R.S.
E. B. Knobel, Esq.
George Knott, Esq., LL.B.
William Lassell, Esq., LL.D., F.R.S.
Albert Marth, Esq.
Edmund Neison, Esq.
A. Cowper Ranyard, Esq., M A.
H. J. S. Smith, Esq., M.A., F.R.S., Savilian Professor of Geometry, Oxford.
E. J. Stone, Esq., M.A., F.R.S., Radcliffe Observer.
Major G. L. Tupman, R.M.A.

REVIEWS.

Researches on the Motion of the Moon, made at the United States Naval Observatory, Washington, by Simon Newcomb, Professor, U. S. Navy. Part I.: *Reduction and Discussion of Observations of the Moon before* 1750. Washington: Government Printing Office. 1878. pp. 280.

The heads of the table of contents are:—1. Historical introduction. 2. Summary of data for determining the apparent secular acceleration. 3. Discussion of narratives of ancient total eclipses of the sun. 4. The Ptolemaic eclipses of the moon recorded in the Almagest. 5. Arabian observations of eclipses, from Caussin's translations of Ebn Jounis. 6. Mode of deducing the errors of the lunar elements from observations of eclipses and occultations. 7. Effect of changes in the lunar elements upon the path of the central line of an eclipse. 8. Observations of Bullialdus and Gassendus. 9. Observations of Hevelius. 10. Observations by astronomers of the French school, between 1670 and 1750, from manuscripts at the Paris and the Pulkowa observatories. 11. Positions of the moon from Hansen's tables, used in comparing the preceding observations with theory. 12. Details of reduction of the occultations. 13. Equations of condition given by the preceding occultations of stars.

14. Observations of eclipses from 1620 to 1724. 15. Discussion of deviations in the moon's mean motion. 16. Motion of the moon's node. 17. Concluding remarks on the value of the secular acceleration deduced in this paper.

An idea may be formed of the amount of patient labour bestowed on this work by the above headings; and even a cursory perusal leads to a high appreciation of the ability and judgment which it displays. It was found necessary to postpone the first part, embracing the mathematical theory of the inequalities of long period in the moon's mean motion, which is still incomplete, and, therefore, the present work is issued as Part I. The subject of the moon's acceleration is explained by Beckett, *Astronomy without Mathematics*, pp. 176—183, 5th edition; by the author of this work in his *Popular Astronomy*, pp. 96—100; and by Grant, *History of Physical Astronomy*, pp. 60—64; but this last comes down only to 1852, and Adams's celebrated paper appeared in 1853. "If we suppose," says Prof. Newcomb, "no deviation in the mean motion of the moon, except that which is due to the gravitation of other bodies of our system, this mean motion would be constant, with the exception of a secular acceleration, the amount of which has been accurately fixed by theory. It is, however, well-known that the secular acceleration given by observation is not the same as that deduced from theory, and astronomers have generally been agreed that the apparent difference may be due to a retardation of the earth's axial rotation. Thus, the apparent secular acceleration will be made up of two parts,—the one a real acceleration; the other an apparent one, due to the change in our measure of time. But when we study the problem more closely, we shall find that the hypothesis of a constant tidal retardation fails to account for the observed mean motion of the moon, and that we must either suppose this retardation variable, sometimes even becoming an acceleration, or we must suppose the mean motion of the moon subject to changes which have not yet been accounted for. (pp. 261.) And he thinks that altogether 'there would be room to hope that we might obtain the secular acceleration from the modern observations alone, with a probable error of scarcely more than half a second [of time] if only the long-period inequalities in the moon's motion were conclusively settled. This is something which is still in the future.' (p. 26.) It is well-known that Hansen introduced the co-efficient 12·″18, founded on an incomplete theory, in constructing his lunar tables, whilst Delaunay fixed it at 6·″176. Prof. Newcomb finds 8·″8 the value which on the whole satisfies the observations he has discussed.

On the hypothesis that the accepted theory of the moon is perfect, and the observed deviations from the theory to be due to inequalities in the earth's axial rotation, Prof. Newcomb gives a table showing the amount in time by which the earth is in advance of an assumed uniformly revolving earth. The table extends from the epoch —687, when the amount of time in question was —70m. to 1875, when it was +14s. And he observes, "These corrections are so minute that their independent detection by existing observations is barely possible. The most promising means of detection is afforded by the eclipses of the first satellite of Jupiter, which have been observed since 1670. Next in order come meridian observations of Venus during several months on each side of her superior conjunction, the discussion of which would be extremely laborious, and would involve a complete re-examination of the theory of the motion of Venus. Transits of Mercury also afford some hope, but, unfortunately, Halley's excellent observation of the transit of 1678 is vitiated by some defect in his clock error, which cannot be investigated for want of data.

If the hypothesis in question is correct, the problem of predicting the moon's motion with accuracy through long intervals of time must be regarded as hopeless, since it cannot be expected that variations in the earth's axial rotation will conform to any determinable law. Success in tracing the deviations in question to the moon itself, and to the theory of gravitation is, therefore, a consummation to be hoped for." (p. 266.)

On the hypothesis that the deviations arise from one or more inequalities of long period in the actual mean motion of the moon, the author finds that the modern observations indicate a time having a period of between 250 and 300 years, and supposing an empirical correction to Hansen's first term depending on the action of Venus, the period of which is 273 years, the observations are quite satisfactorily represented. The additional diminution of 10″ per century in the mean motion of the moon at the present time will necessitate a further diminution of 0·″5 in the value of the secular acceleration in order that the ancient observations may still be well represented. The best mean representation of the ancient observations is given with the acceleration 8.″3. The correction to the centennial mean motion given by modern observations is found to be −29·″2.

In his concluding remarks, where the evidence on both sides is clearly and fairly presented, Prof. Newcomb says, that "apparently, we can hardly avoid accepting one of these propositions:—Either the recently accepted value of the acceleration and the usual interpretation of the ancient solar eclipses are to be radically altered, the eclipse of —556 not having been total at Larissa, and that of —584 not having been total in Asia Minor; or the mean motion of the moon is, in the course of centuries, subjected to changes so wide that it is not possible to assign a definite value to the secular acceleration." In regard to the material which "though rejected, may not be valueless, the objection to the use of the Chinese and Assyrian eclipses was substantially the same as that to the classical eclipses; the accounts inspired no reasonable certainty that the eclipses were severally total at definite points of the earth's surface."

The imperfection of Hansen's tables of the moon was proved by Prof. Newcomb in 1870, and that their correctness since 1750 had been secured only by sacrificing the agreement with observations previous to that epoch, so that about 1700 these tables deviated more widely from observations than those which they superseded. He also showed that at the time of writing, the moon was falling behind the tabular position at a rate which would speedily cause a very serious error in the representation of the tables. Altogether, it appeared that notwithstanding the immense improvement which Hansen had made in the accuracy of the inequalities of short period, the theory of those of long period was no nearer such a solution as would agree with observation, than where it was left by Laplace. The work of re-investigating the subject was made a part of the author's official duty at the Naval Observatory, and the expenses of two computers being allowed by Congress, the first part of these researches is now before the astronomical world; interesting to all, but pre-eminently to those whose attention has been directed to this intricate and laborious department, and whose acquirements enable them to appreciate and follow the labours of the select few who are advancing the knowledge of it. To these last it must be of almost inestimable value.

In a journey in Europe in 1871, Prof. Newcomb discovered old observations in the archives of the observatories of Paris, and of Pulkowa, hitherto either unknown or unreduced. The toil of working up these for the most part rough and imperfect notes must have been very great; but it has brought to the present investigation material hitherto least known. The most valuable of the Arabian observations in the *Livre de la Grande Table Hakémite*, translated from an Arabian manuscript by Caussin, nearly forgotten, have, it is believed, been for the first time fully compared with modern tables. The ancient total eclipses of the sun, so much discussed during the present century, are thrown aside, but the reasons are fully stated, and those who may differ in opinion from the author in this matter will have no grounds for complaining of partiality or one-sidedness. He approached, he says, the subject without any bias whatever, unless a general distrust of the scientific precision of ancient authors may be regarded as a bias, and the various considerations which presented themselves to his mind on examining these records are reproduced as exactly as possible. While the result of the examination of ancient solar eclipses has seemed to him to justify his general distrust, that of the lunar eclipses in the *Almagest* has not.

In conclusion, the author places on record his obligation to the skilled assistants, without whose help the completion of the work would not have been possible. These were Mr. Parker Phillips, Mr. John T. Hedrick, Mr. John Meier, and Mr. W. F. McK. Ritter. Mr. D. P. Todd took an active part in passing the work through the press.

We should add that these Researches are interspersed with curious and interesting particulars concerning some of the old astronomers, and their observations.

Annales de l'Observatoire Royal de Bruxelles, publiées aux frais de l'état. *Nouvelle série, Astronomie.* Tome I. et Tome II. Bruxelles: F. Hayez, imprimeur de l'Academie Royale. 1878.

The first volume contains *Uranometrie Generale*, by J. C. Houzeau, director of the observatory; Introduction, pp. x. 54; Catalogue of stars, pp. 55—117; Maps; Tables of the constants of astronomy, by J. C. Houzeau, pp. xiii. 271. These annals commenced in 1834 with the origin of the observatory, and have been uninterruptedly continued by its founder A. Quetelet. The meteorological part is henceforth to be kept separate. The uranometry is the result of the sole labour of M. Houzeau, and embraces all stars visible to the naked eye in both hemispheres. Stationed near the equator, where he could transfer himself readily from one hemisphere to the other, he made a chart on which he laid down all the stars without distinction of magnitude. He then noted, without any reference to previous observers, and inscribed the magnitudes, each star being generally observed two or three nights following. Half magnitudes are noted both in the catalogue and in the charts. In the latter the varied brightness of the Milky Way, and 13 conspicuous nebulæ and clusters are indicated by curves of equal intensity of bluish tint, like curves of level in topographical maps. By a method which he explains, the author fixes the position of the N. pole of the Milky Way for the equinox of 1880, 0 in R.A., 12h. 49m., Decl. +27° 30′, which differs little from Struve's determination. The medial circle of the Milky Way is very nearly a great circle of the sphere. The total number of the stars is 5,719. The S. hemisphere is a little less rich than the N. in stars visible to the naked eye, the difference being about a hundred. The inequality is most sensible in sixth magnitude stars. In a total of 5,640 the N. hemisphere contains 200 more than the S. The epoch of the work is 1880, 0. The maps are very clearly and well drawn, and there are investigations about the distribution of the stars. Observations made in the fine nights of the tropics, when simultaneous determinations of magnitude of many stars at short intervals are possible, have a great advantage over isolated estimations. This fine work is thoroughly original, and not in the least touched up by recourse to other books. The author does not pretend it is free from faults, but he has evidently taken great pains, and now in succession to the similar publications by Argelander, Heis, and for the southern heavens Behrmann, he has presented us with a uranometria which cannot fail to be valued as it deserves to be by astronomers of every degree. The report on the Uranometrie Generale was presented to the Royal Academy of Belgium, 1st July, 1876, by M. le General Liagre, whose *précis* will be read with interest. In his concluding remarks the General says, "The Academy must felicitate itself on the great work just accomplished by one of the most laborious and most devoted to science of its members. An inventory, exact, detailed, actual, of all the riches of the heavens, prepared in thirteen months by one man, is a veritable monument raised to observational astronomy; it cannot fail sooner or later to be fruitful in results, for as Pliny said of the catalogue of Hipparchus, a work of this sort is a heritage bequeathed to posterity: *Cœlum posteris in hereditatem relictum.*"

The remainder of this volume is the Répertoire or Assemblage of all numerical particulars connected with the solar system, comets, stars, &c., drawn from a multitude of ancient as well as modern books, to each of which reference is carefully given. Such a mass of the "constants of astronomy" was probably never before brought together, and it will be found very useful. It is divided into 19 chapters. Each planet with its satellites has a chapter to itself, and reference to any desired subject is made ready and easy. M. Houzeau well remarks, "An abstract of the works mentioned by their titles and other particulars would of itself form an astronomical bibliography. It would be a kind of select astronomical library, not embracing, it is true, the entire domain of astronomy, but at any rate that of numerical determinations."

Vol. II. contains Introduction, observations of stars in 1873—74—75, made with the transit instrument and mural circle, and three catalogues of the stars observed in those years, comprising respectively 4,150, 2,058, and 1,983 stars—in all, 8,191. These catalogues are preparatory to the formation of the final catalogue, began in 1857, which will comprise about 10,000 stars. The principal object being a general revision of proper motions, which amount to at least the tenth of a second of arc a year. The transit observations were made by M. Ernest Quetelet, and the reductions by MM. Ernest Quetelet and L. Estourgies. The meridian telescope is by Gambey. The observations with the mural circle were made by MM. Ernest Quetelet, Hooreman, and Estourgies, and the reductions by MM. Ernest Quetelet and E. Goemans. This instrument is by Troughton and Simms. This volume also contains: Observations on the physical aspect of the planet Mars during the opposition of 1877, by L. Niesten, assistant astronomer at the observatory, with delicate chromo-lithographs, and Proctor's map. For these observations the equatorial of 6-in. aperture, with powers 90 to 450 was used, but in particular powers 270 and 180 were employed. This portion is also published in a separate form, and to those especially who are occupied with this planet it will be very interesting.

Recherches sur les Couleurs des Etoiles doubles. Par M. L. Niesten, astronome à l'observatoire de Bruxelles. Academie Royale de Belgique (extrait des Bulletins, 2me série, tome XLVIII., No. 1, Janvier, 1879,) pp. 21,

On the ground of the connection between the periodicity of the solar spots and the positions of the planets, Mr. Niesten inquires whether the sun's influence may not also affect the colours of the latter, and also whether such a correlation may not also exist in certain systems of double stars, whose changes of colour may be due to a physical alteration of the gaseous envelope surrounding them. He has drawn up a table exhibiting the calculated orbits of the principal systems, the colours of their components at different epochs, as noted by various observers, the epoch of periaster, and the period of revolution. His conclusions are: 1. That in systems of well recognised orbital movement, and especially those of short period, the two components have generally the same colours, yellow or white. 2. In systems the colours of which have been sufficiently observed to compare their colours with the place of the satellite in its orbit, the principal star is white or pale yellow when the companion is at its periaster, whereas in other positions it is yellow, golden yellow, or orange. 3. That in those systems the companion follows the principal star in its fluctuations of colour and often exceeds it in depth of colour in proportion as it recedes from the periaster, where its light in the greater number of cases is white like the principal star. 4. That the same equality of tones in the principal and secondary stars is found in the pairs which have a rectilineal movement and those of orbital movement and long periods of revolution. 5. That in perspective groups (*i e.*, only optically associated) the companion is almost always blue. M. Niesten thinks that when we shall become better acquainted with the orbits of double stars, and the colours of the planets, it may be possible to deduce certain consequences with greater probability than he has been able to do by reason of the paucity of observations at present available. His paper deserves, and will no doubt receive, attention: though it may be doubted perhaps if the influence of the planets upon the sun's spots has been yet made out to a demonstration.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE TRAPEZIUM IN ORION.

This wonderful system of stars—partly on account of its remarkable arrangement, partly on account of its brightness and the consequent ease with which it can be scrutinized, partly, too, owing to its peculiar position with reference to the vast nebula in which it lies, and partly, no doubt, from the fact that there exist within, or very near it, at least two (and possibly several) minute stars suitable in some respects for testing telescopes—has attracted the attention alike of the professional and the amateur astronomer for more than two centuries. It is, along with the famous nebula, one of the show objects for visitors to observatories: indeed, it has been little else in our case hitherto, owing principally to the facts that no sensible change in the relative positions of the stars has been detected since the observations of Σ, and that we can so rarely obtain a good view of the fifth and sixth stars.

Having recently had occasion to look into the history of this system, with especial reference to the difficult objects known as the fifth and sixth stars, I have been tempted to re-open the question of the variability of these stars, and also to make an appeal to amateur astronomers to aid in clearing up this and some other points connected with the trapezium. And, in order that those to whom the appeal is made may be able conveniently to form a correct judgment as to how far the proposed investigation is worthy of their attention, I subjoin the principal results of observation, so far as they are known to me.

And, firstly, let us take the question of the variability of the two faint stars, the fifth and sixth. The magnitudes assigned to these objects are as follows: the fifth, 11·3 (Σ.), 10 (O. Σ.), 10¾ (Dawes); the sixth, 11·3 (O. Σ.), 12 (Dawes).

The differences here are not very remarkable, if we consider the difficulty of the objects. But when we come to compare the remarks of different observers on the same point, even when all the difficulties of the case are fairly allowed for, the legitimate conclusion seems to be that until a more systematic examination of these stars has been made under favourable conditions as to latitude and sky, we cannot regard the question as settled. Let us examine a few of the remarks here referred to. Σ. thought the fifth was not a variable. Sometimes he could not see it at midnight, and yet at others it came out with the greatest distinctness in strong twilight; the sixth he never could see at Dorpat, and he was of opinion that the visibility of the fifth depended entirely on the state of the sky.

Sir John Herschel was quite sure the fifth was a variable of long period; in 1828 he thought it "could not equal the 17th magnitude" (of his scale), while in 1834 he estimated it of the 12th; and to the sixth star he assigned the 14th mag. Moreover, he thought the fifth a *new* star, on the grounds that neither his father nor Σ. before 1826, nor himself and others who had most carefully examined the place where the star was found, had detected it.

Dawes, on the other hand, always thought this star maintained a uniform brightness, but that the sixth was a variable to the extent of 1 or 1½ magnitude.

Lassell found the fifth *less bright* than the sixth in 1856, and O. Σ.'s observations previously to 1857 point in the same direction; after that year the sixth was less bright than the fifth.

Secondly, if we try to gather from the observations on record what instrumental aid is necessary to enable us to to see these objects well, some surprising facts come out, *e.g.*:

Sir John Herschel, at the Cape of Good Hope, thought it very good seeing when his 18-inch mirror, stopped down to 12-inch, enabled him to see the sixth star well. On the other hand, De La Rue has seen both the fifth and sixth with his 4⅛-inch refractor. Noble can always see the fifth, and occasionally the sixth also, with a 4·2-in. Burnham's fine eye can see them with an aperture of 3¾-inch, and the same observer can always see both stars with his 6-inch glass on suitable nights.

At Bermerside we can rarely see the fifth *well*, and we can seldom see the sixth at all. It is on the whole pretty clear, therefore, that state of sky and sensibility of eye have much to do with the explanation of the results, and the kind of investigation before suggested can alone decide the share variableness in the light of the star has in causing the discrepancies in the observations. And we further see that the observers who may undertake such a systematic scrutiny do not need large instruments; a telescope of very moderate size, a good eye, and above all a fine sky, are all that is necessary.

Lastly, it would be easy to combine with these observations some others which would clear up some interesting points with reference to the trapezium and the adjacent nebula, *e.g.*, the variability of any of the four principal stars: the existence of minute stars within or near the trapezium; the variability of such suspected stars, if found actually to exist; the variability in brightness or change in form of the nebulous matter near the trapezium, and so on. With regard to the second point, it is well known that much has been said. De Vico, Bond, Porro, Huggins, De La Rue, Lassell and others have all seen minute stars either within the trapezium or on its borders. But it must also be noted that Burnham after repeated careful examination with apertures from 6-inch to 18½-inch has never been able to glimpse any other stars than the well-known six. So also with regard to the nebula; it is more than probable that careful observations of the brightness of the portions adjacent to the trapezium, if continued for some years on a good and uniform plan by the same eye, would yield results of considerable interest.

Mr. Edward Crossley's Observatory, J. GLEDHILL.
Bermerside, Halifax.

THE RED SPOT ON JUPITER.

Since August 31st, 1879, this beautiful object has been frequently observed, and occasionally roughly measured at this observatory. The measures are rough owing to the bad sky which generally prevailed during the autumn and early winter. The "spot" was measured when central on the disk.

Aug. 31st, 1879. Length about 14″. Greatest breadth about 3″. Bad definition.

Nov. 7th, 1879. Length about 11½″. Greatest breadth about 3″. Bad sky.

Nov. 12, 1879. Length about 13″. Greatest breadth about 2½″. Much boiling.

Nov. 14th, 1879. Length about 12″. Greatest breadth about 3″. Distance of centre of "spot" from south pole of Jupiter about 12″.

Nov. 29th, 1879. Length about 12″. Distance of centre from south pole about 12″.

Dec. 1st, 1879. Length about 12″. Distance of centre from south pole about 12″. Fair definition.

Dec. 6th, 1879. Length about 12″. Distance of centre from south pole about 12″. Good definition.

Jan. 28, 1880, 5.30 p.m. Length of "spot" about 10½″. Distance from south pole about 11″. Bad definition.

Mr. Edward Crossley's Observatory, J. GLEDHILL.
Bermerside, Halifax.

STAR IN CANIS MAJOR.

Sir,—I have never seen any statement as to what it is that Baxendell has discovered about his star in Canis Minor. Is it a new star altogether? or is it only new as an orange star? With my telescope I cannot perceive any colour. Or has Baxendell discovered an already known star to be a variable star? for it certainly is variable. Fuller information than has as yet appeared in any of the astronomical periodicals will oblige,

Yours truly,
Sunderland: Feb. 5, 1880. T. W. BACKHOUSE.

NOVA CYGNI AND NEIGHBOURING STARS.

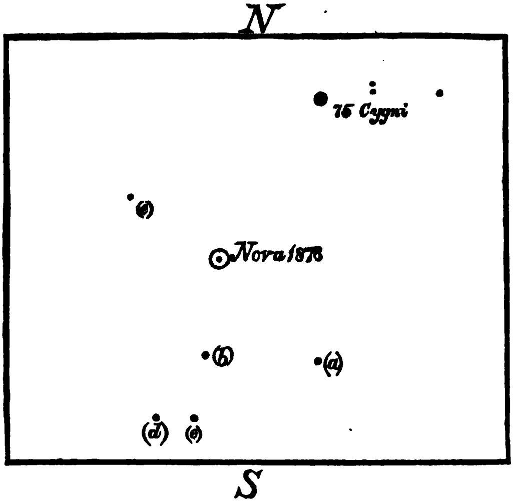
Sir,—In reply to Mr. Backhouse's question in the *Astronomical Register* for Feb., 1880, I regret to say that I have not found out much further about the stars he refers to. With reference to the stars lying about 23′ *s. p.* the Nova, I certainly did glimpse a small star near the place with my binocular in January, 1877, but whether this star was really identical with Lalande 42306 (as I thought at the time) I cannot say. I find from my note-book that a few days after, with 3-inch refractor, I estimated this star as 10 mag., and also found a small star about 11 mag., close to the place of Lalande 42360. I have since read somewhere (I forget exactly where, but I think in *Nature*) that Argelander considered that Lalande 42360 was identical with Lalande 42383. (The difference of N.P.D. as given in Lalande's Catalogue is only 2″·5.)

I send a sketch of the brighter stars in the vicinity of the Nova, which I made in January, 1877.

(*a*) is Lalande 42306 (8 mag.). Harding 8 mag. Gore 10 mag.
(*b*) is a small star (about 9 mag.), not given by Lalande or Harding.
(*c*) is Lalande 42360 (7 mag.) not given by Harding.
(*d*) is Lalande 42383 (8 mag.). Harding 8 mag.
(*e*) a small star not given by Lalande or Harding.

Ballisodare, Co. Sligo:
Feb. 5, 1880.

Yours truly,
J. E. GORE.



METEORS OF OCTOBER 28, 1866.

In recently looking over my note-book I met with the following observation:

> "1866, Oct. 28. Very fine night. A great many shooting stars; one very bright at 6h. 15m., and another at 9h."

I shall be very glad to hear if any of your readers were at work that night, and if so whether they remarked the unusual frequency of meteors referred to. I have no distinct recollection of my observation, and omitted at the time to record the numbers and directions of the meteors, but possibly the shower may have been witnessed elsewhere and more particularly recorded. Though the date does not correspond with either of the well-known meteor epochs, it appears to deserve attention, for several radiant points have been determined on the night of October 28. One of Schiaparelli and Zezioli's showers is given at 110° + 70° for Oct. 28, 1868, and Sawyer, at Boston, Mass., observed an active display of meteors from a point at 356° + 40° in the early evening of October 28, 1877.

Bristol: Feb. 5, 1880. W. F. DENNING.

Erratum.—The beginning point of the first meteor in the list of "Leonids seen at Bristol, Nov. 13, 1879" (*Register*, No. 205, p. 16), should be at 90° + 12° instead of 10° + 12°.

THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—I had no desire at the annual meeting of the Royal Astronomical Society on February 13 to make a speech, for it would not have been regular to have done so at the time when I rose to ask the President the question which I put to him. Nevertheless, I think it may be well that I should take an opportunity of expressing my object in seeking to bring about an alteration in the bye-laws.

There prevails amongst the Fellows generally, especially amongst those living at a distance from London, a feeling that the Council is not, except in name, a representative body; that it is too much self-elective, and is not the deliberate choice of the Fellows at large. In other words that a dozen or two of men annually choose the Council, in derogation of the electoral privileges of 500 and odd distant Fellows. This is a notorious fact, and, as we have seen during the last few years, is very prejudicial alike to the interests of the Society and of the science of astronomy generally.*

I had proposed to myself the other day no further task than that of re-modelling the bye-laws which deal with the annual election of the governing body. The change which I contemplated was the establishment of voting by proxy, coupled with a more open system of nominating officers, for the present system of a "house-list" gives rise to interminable jobbery and cliquism. It was entirely in consequence of a conversation which I had with Lord Lindsay and Mr. Barrow that I expanded my proposal into a scheme for a general revision of all the bye-laws *en masse.* I fully believe that such general revision is necessary, but I had no desire to open up so large a subject. I may add, I do not intend that my limited scheme of improved election arrangements should be swamped or indefinitely postponed, pending the general revision of all the bye-laws. A scheme for altering all may readily be made the pretext for a long postponement of the very small and easily accomplished revision of the election bye-laws, which could be brought about by the alteration of merely a few dozen words in the present bye-laws.

I should be glad if you would publish this note, because I have reason to know that a large number of Fellows living at a distance from London have long been very anxious for proxy voting, or something like it, to be established as soon as possible.

Northfield, Eastbourne: Your obedient servant,
Feb. 16, 1880. G. F. CHAMBERS.

LESSONS IN SELENOGRAPHY.

By the Rev. W. J. B. Richards, F.R.A.S., and W. R. Birt, F.R.A.S.

These lessons are intended to assist beginners in studying the configurations and conditions of the moon's surface, by pointing out the most striking features of our satellite, and indicating, in the opinion of the authors, the best methods of becoming acquainted with them.

Chapter I.—Identification of Objects.

The first step in practical Selenography is the identification of the most conspicuous objects on the moon's surface. For this purpose the most important requisites are telescopes and maps. For a beginner we should recommend, in the first instance, a small telescope, and we cannot do better than refer the reader to the opening pages of Webb's most excellent and useful work, "Celestial Objects for Common Telescopes," (of which a new edition is expected shortly), for some most interesting remarks on the management and use of telescopes. Some good work has been effected with a clear aperture of *two* inches only, and as this is pretty well the smallest working aperture, or clear space, which receives the light from the object, no one need despair of rendering even valuable assistance to the progress of Selenography.

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* [We print Mr. Chambers' letter, although we do not think that the assertions contained in it are all borne out by fact. The method of electing the Council of the Astronomical Society is the same as in all the other learned societies.—Ed.]

We propose in Chapter III. to introduce some remarks on the maps most suitable for the enquiry in its various stages. In the meantime, for a student who commences early in life with the prospect of attaining an age exceeding, it may be, fifty years, we should recommend to use with a small telescope the map which is prefixed to these lessons as a frontispiece; it has been very carefully reduced from Beer and Mädler's large map. The observer should first provide himself with a note-book of sufficient size to allow of inserting sketches in the margin. Each memorandum in the book should be numbered from 1 onwards and headed with the year and month, day, hour, and minute, thus: No. 1, 1880, Jan. 20, 13h. 30m., then should follow the day elapsed of the Julian period, taken from the *Nautical Almanac*, p. 492, last column, viz., 2407737. The next particular, the telescope employed, especially the aperture, also the power of the eyepiece; then may follow some notes of the state of weather, &c. Collecting these items, the record stands thus:

		d.	h.	m.	
No. 1	1880, Jan.	25	13	30	D. J. P. 2407737
	Telescope.			Aperture.	Eyepiece.
	Weather, fine, frosty.				Definition good.

The beginner, provided with his telescope and note-book, may make his first attempt in identification by noting, especially during the months between December and June, the largest grey plain nearest the western edge (limb) of the moon when the moon's age is about three days. He should note in plain, intelligible language, the general appearance of this plain, marked A on the map, and known as the *Mare Crisium*. There are two conspicuous craters on its surface, *Picard* No. 21 and *Peirce;* at so early an illumination notice particularly the interior and exterior shadows, recording the extent of the interior in decimal parts of the apparent shortest diameter of each; also any peculiarity that may arrest attention, either on the *Mare* or its mountain border.

We have recommended the first observation to have been made at or about three days of the moon's age; it would be well to notice the objects through or near which the boundary between light and darkness (terminator) on the moon's surface passes, and make a record of at least two in the upper part or southern hemisphere, and two in the lower part or northern hemisphere. By pursuing the same course, moon's age four days, the observer will be able to ascertain the extent that daylight has advanced on the moon in the preceding 24 hours. He may next proceed to identify the *Mare Tranquilitatis* (F), thus proceeding from west to east until he arrives, just before full moon, at the eastern edge or limb, near which he finds the dark floored walled plain, *Grimaldi*. In this process from night to night many considerable mountain ranges will be met with, most of which are inserted on the small map, and should be recorded when identified in the note-book. If any objects are seen which cannot be found on the map a special note and sketch should be made of it.

(*To be continued.*)

ASTRONOMICAL OCCURRENCES FOR MARCH, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Mon	1		Sidereal Time at Mean Noon 22h. 38m. 39·91s.			Regulus 11 21·5
Tues	2		Sun's Meridian Passage 12m. 13·13s. after Mean Noon			11 17·5
Wed	3	11 16	☾ Moon's Last Quarter	Jupiter's satellites invisible through nearness to the Sun.		11 13·6
Thur	4					11 9·7
Fri	5					11 5·7
Sat	6					11 1·8
Sun	7					10 57·9
Mon	8	0	Conjunction of Moon and Venus 3° 20′ S.			10 53·9
Tues	9					10 50·0
Wed	10	12 46	● New Moon			10 46·1
Thur	11	0	Conjunction of Moon and Jupiter 6° 26′ S.			10 42·1
Fri	12	4 12	Conjunction of Moon and Mercury 3° 3′ S. Conjunction of Moon and Saturn 7° 52′ S.			10 38·2
Sat	13	7 35 8 30	Occultation of 101 Piscium (6) Reappearance of ditto			10 34·3
Sun	14		Sidereal Time at Mean Noon 23h. 29m. 55·10s			10 30·3
Mon	15	8	Conjunction of Jupiter and Sun Saturn's Ring: Major axis=36″·41 Minor axis= 6″·76 Illuminated portion of disk of Venus=0·855 Illuminated portion of disk of Mars=0·899			10 26·4
Tues	16		Sun's Meridian Passage 8m. 38·35s. after Mean Noon			10 22·5

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Regulus
Wed	17	6 13 7 33 12 30 13 15 12 35 12	Occultation of B.A.C. 1518 (6) Reappearance of ditto Occultation of 103 Tauri (6) Reappearance of ditto Near approach of Mars Conjunction of Moon and Mars 0° 23′ S.			10 18·5
Thur	18	12 36 6 13 7 25	☽ Moon's First Quarter Occultation of 132 Tauri (5½) Reappearance of ditto	Jupiter's satellites invisible through nearness to the Sun.		10 14·6
Fri	19					10 10·7
Sat	20					10 6·7
Sun	21	7 16 8 21	Occultation of d^2 Tauri (6) Reappearance of ditto			10 2·8
Mon	22					9 58·9
Tues	23					9 54·9
Wed	24	13 36 14 20	Occultation of p^2 Leonis (6) Reappearance of ditto			9 51·0
Thur	25	11 10 12 8	Occultation of B.A.C. 4006 (6) Reappearance of ditto			9 47·1
Fri	26	1 23	○ Full Moon			9 43·1
Sat	27					9 39·2
Sun	28	12	Inferior conjunction of Mercury and Sun			9 35·3
Mon	29	11 46 17 39 18 41	Occultation reappearance of 42 Libræ (5½) Occultation of B.A.C. 5253 (6) Reappearance of ditto			9 31·3
Tues	30	13 6	Occultation reappearance of 25 Scorpii (6)			9 27·4
Wed	31					9 23·5
APRIL Thur	1					9 19·5

THE PLANETS FOR MARCH.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	23 41 15	S. 2 13½	5″·6	1 2·4
	9th	0 24 34	N. 4 21	6″·8	1 14·2
	17th	0 44 46	N. 8 10	8″·8	1 2·8
	25th	0 36 33	N. 7 36	10″·6	0 23·2
Venus ...	1st	20 43 9	S. 18 17½	12″·8	22 0·9
	9th	21 22 46	S. 15 53½	12″·4	22 8·9
	17th	22 1 22	S. 13 1½	12″·0	22 16·0
	25th	22 39 1	S. 9 46	11″·6	22 22·1
Mars ...	1st	4 23 10	N. 23 29½	8″·4	5 43·6
	9th	4 40 58	N. 24 5	7″·8	5 29·9
	17th	4 49 28	N. 24 33	7″·4	5 16·9
	25th	5 18 31	N. 24 54	7″·0	5 4·4
Uranus ...	1st	10 35 10	N. 9 47½	4″·4	11 54·5
	13th	10 32 15	N. 9 59	4″·2	11 5·4

Mercury sets an hour and a quarter after the sun, the interval increasing to the middle of the month, after which the interval rapidly decreases.

Venus rises an hour and a quarter before the sun on the 1st, the interval decreasing.

Mars sets about two hours after midnight at the beginning of the month, and then earlier each night.

Saturn sets an hour and a quarter after sunset on the 1st, the interval rapidly decreasing.

SATELLITES OF URANUS.

Major and minor semi-axes *a* and *b* of the apparent ellipses described by the satellites, and position-angles p_0 of the major axes:

	Ariel.		Umbriel.		Titania.		Oberon.		
1880.	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	p_0
	″	″	″	″	″	″	″	″	°
Mar. 5	15·27	2·41	21·28	3·36	34·90	5·51	46·67	7·37	13·68
15	15·22	2·52	21·23	3·51	34·82	5·75	46·57	7·69	13·61
25	15·18	2·61	21·15	3·63	34·69	5·96	46·39	7·97	13·54

The satellites will be at their greatest northern and southern elongations N. and S. (in position angle 13°·6 and 193°·6), and at their inferior and superior conjunctions (in pos. 283°·6 and 103°·6) about the following hours, G.M.T.:

	Ariel.		Umbriel.		Titania.		Oberon.
1880.	h.		h.		h.		h.
Mar. 1	17 S.	Mar. 1	16 N.	Mar. 1	5·8 N.	Mar. 1	15·9 sup.
2	23 N.	3	18 S.	3	10·1 inf.	5	0·7 N.
4	6 S.	5	20 N.	5	14·3 S.	8	9·5 inf.
5	12 N.	7	21 S.	7	18·6 sup.	11	18·3 S.
6	18 S.	9	23 N.	9	22·8 N.	15	3·2 sup.

Ariel.				Umbriel.				Titania.				Oberon.			
		h.				h.				h.				h.	
Mar.	8	0	N.	Mar.	12	1	S.	Mar.	12	3·1	inf.	Mar.	18	12·0	N.
	9	7	S.		14	3	N.		14	7·3	S.		21	20·8	inf.
	10	13	N.		16	4	S.		16	11·5	sup.		25	5·6	S.
	11	19	S.		18	6	N.		18	15·8	N.		28	14·4	sup.
	13	1	N.		20	8	S.		20	20·0	inf.		31	23·2	N.
	14	8	S.		22	10	N.		23	0·3	S.	Ariel.			
	15	14	N.		24	11	S.		25	4·5	sup.	Mar.	24	10	S.
	16	20	S.		26	13	N.		27	8·8	N.		25	16	N.
	18	2	N.		28	15	S.		29	13·0	inf.		26	22	S.
	19	9	S.		30	17	N.		31	17·2	S.		28	4	N.
	20	15	N.	April	1	18	S.	April	2	21·5	sup.		29	11	S.
	21	21	S.										30	17	N.
	23	3	N.										31	23	S.

A. M.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN MARCH, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Censorinus (printed in the last list on p. 50, line 2, *Cinsoninus*). This crater we described as of 9° of brightness, and recommended that it should be compared for brightness with several other craters. This month we would call attention to several of its neighbours that are laid down on Neison's map XXII., and we should advise a perusal of his text. The crater itself is opened upon the eastern edge of a highland bordering the southern part of the *Mare Tranquillitatis*. On the west, about one diameter of *Censorinus* from it, is a crater, marked *a*, resembling it in size, and stated by Neison to be 5° bright. Neison also gives on the west border of the *Mare Tranquillitatis*, SSE. of Censorinus. a crater of about the same size, with a smaller craterlet SE. of it; this crater and craterlet are undesignated. Carrying the eye from *Censorinus a*, past the double crater, or crater and craterlet to *Torricelli*, it encounters a crater marked B on Neison's map, and immediately east of it is a peak marked γ, ESE. of the crater and peak is another crater, also marked B, with a smaller crater SSE. of it, and nearly in a line with *Torricelli*. On the north of these craters will be seen a cleft or rill marked ψ.

On becoming acquainted with the objects above mentioned, the observer will be prepared to examine the whole of the district around *Censorinus*, and identify all the lettered objects on the map. A notice of such as may be identified, and of those not detected in the search, if sent to the address below, will meet with every attention.

Mare Crisium. Although the surface of this formation has been carefully scrutinized 14 years ago, and a correct list of the craters seen on it published, it should be carefully examined at the present time, not only for ascertaining if these craters are now in existence, but also for determining the forms and directions of the ridges that characterize it. Two are well shown by Schröter in T XXXIII, fig. 2. One which runs nearly parallel with the eastern border, and is about equally distant from the border, the other which runs from the north border to the south border, and starts from a crater close to the north border, slightly sinuous, passing west of the largest crater, Picard. At a point SW. of this crater, the ridge sends off a branch towards the SE., from which the two have obtained the name of "Schröter's forked ridge." It is intended to make a fresh examination of the ridges of this and other *Maria*, and drawings of them, especially of the "forked ridge," will be very acceptable. They may be forwarded to the address below.

Water Lane, Stratford, E.:
February 11, 1880.

Books received.—The Fourteenth Report of the Board of Visitors of the Observatory at Victoria. 1879.—Vol. XI., Part II., of the Harvard College Observations—Greenwich Astronomical Observations. 1877.—Astronomie Populaire, Description Générale du Ciel, illustrée de 360 figures, planches en Chromolithographie, Cartes Célestes, etc. Par Camille Flammarion. Paris: 1880.

Erratum on p. 47, line 11, for *past* read *preceding*.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1879.

Dallmeyer, J. H.
Elliott, R.
Erck, Dr. W.
Monk, Dr.

To May, 1880.

Brothers, J.

To Dec., 1880.

Baxendell, J.
Duncan, J.
Garnett, W.
Johnson, R. C.
Hunt, G.
Langley, Professor
Lettsom, W. G.
Markwick, E. E.
Prince, C. L.
Richards, Rev. W. J. B.

To Jan., 1881.

Hutchings, Rev. R. S.

To Dec., 1882.

Glaisher, J. W. L.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to John C. Jackson, at Lower Clapton, London, E.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 208. APRIL. 1880.

ROYAL ASTRONOMICAL SOCIETY.

Session 1880—81.

The first Meeting of the New Session was held at the Society's rooms, Burlington House, Piccadilly, on Friday, March the 12th, 1880.

J. R. Hind, Esq., F.R.S., *President*, in the Chair.

Secretaries—W. H. M. Christie, Esq., and J. W. L. Glaisher, Esq.

The minutes of the January meeting were read and confirmed.

Mr. Christie announced that 86 presents had been received by the society since their last meeting, amongst which were two volumes of the "Philosophical Transactions," which had been given by the Royal Society.

The Rev. C. S. Harris, M.A., of Mepal Rectory, Ely, and
The Rev. J. J. M. Perry, M.A., of St. Paul's Vicarage, Alnwick,
were duly elected fellows of the society.

The President said: I hold in my hand a list of twelve fellows of the society who are in arrears more than eight guineas, and according to the bye-laws their names will be suspended in the meeting room.

Mr. Glaisher read a paper by Mr. J. Gledhill *On the Phenomena of Jupiter's Satellites* observed at Mr. E. Crossley's observatory, near Halifax. The instrument with which the observations were made was an equatorial of 9¼ in. aperture; usually a power of 240 had been made use of, and occasionally a power of 330 and 470.

Mr. Glaisher also read a paper *On the Satellites of Mars*, by Professor Asaph Hall, and a paper on the same subject by Professor Pritchard, who had observed the satellites with the Grubb equatorial of 12¼ in. aperture. In a letter to Mr. Glaisher Professor Pritchard said: "I think it a loss of time and a trial of patience to look for such minute objects with a telescope of 12 or 15 in. aperture, and I would suggest that they should be relegated to such instruments as Mr. Common's."

Mr. Christie said: Several papers have been received from observers in the southern hemisphere relative to the great comet which has been seen there. I will read some extracts from two letters which have been received by the Astronomer-Royal from Mr. Gill at the Cape. One commences—"We have got a comet by the tail; I am sorry to say we have only got him by the tail." It describes observations from the 3rd of February to the 15th, and states that no nucleus was observed till February 8.

Mr. Christie exhibited a globe on which he had drawn the comet in position. It must have been a striking object at one time, as the tail was 90° in length. Questions had arisen as to how it was this comet had not been seen in the northern hemisphere, but there was a good reason for it, because the tail was nearly parallel to our horizon at sunset. Several other letters had been received on the same subject. One from the chaplain and naval instructor on board a ship stationed at Monte Video. and a paper from Lieut. Gwynne, on board the same ship. These observations differed slightly from those of Mr. Gill, but not more than was to be expected. There was also a newspaper account of the observations of the tail, and a letter from the Governor of St. Helena, who said the tail reminded him of the comet of 1843, but he saw no nucleus. In Mr. Gill's telegram there was some ambiguity about the position of the comet. According to the code adopted by the Vienna academy the figures given by Mr. Gill, 08 58, would refer to Cape mean time, but it appeared later that they referred to the right ascension of the comet as expressed in arc, but in that case there should have been another 0 before them—there should have been five figures. In the account sent to the newspapers the interpretation adopted was that the figures meant 8h. 58m.; but in the telegrams sent to observers the actual words of the telegram were repeated without any attempt at explanation. The reason for sending the interpretation to the newspapers was that a comet in R.A. 8° 58′ would not be visible in this latitude.

Lord Lindsay said he had thought the figure was intended for the Cape mean time, because in a letter Mr. Gill had referred to an observation at 8h.·30.

Lord Lindsay then read a communication on the same subject from Mr. Eddie.

Mr. Christie: All these communications have come in so late that it is difficult to form any opinion of the value of the observations.

The President: I may be allowed to supplement what has been said about this comet by saying that in a note I have received from Mr. Gill he mentions that he has made a first approximation to an orbit of the comet. *It appears to be inclined at about 88° to the Ecliptic*, and to have passed close to the sun.* It is rather a curious circumstance that nearly all the comets which have been observed in the southern hemisphere, and of which we have not caught a glimpse in these latitudes, have passed very close to the sun, and in a great majority of cases they have had a large inclination. I do not know that there is anything that would cause invisibility in this country on that account; but it is the case with the large comets of the seventeenth century and eighteenth century, and the comets that have been observed this century.

Mr. Glaisher read a paper entitled *Observations of Mimas and occultation of Rhea,* 1879, *with the* 26-*in. equatorial of the U.S. Naval Observatory, Washington*, by Professor E. S. Holden.

Mr. Glaisher also read a paper *On the disappearance of Jupiter's satellites on Oct.* 15, 1883, by S. J. Johnson.

Mr. Knobel: Has not the substance of Mr. Johnson's paper appeared somewhere else this week? I have read it, I think.

Mr. Glaisher: It came to the society as a new paper. I know nothing of it.

Mr. Knobel: I think it was in the *Observatory* that I saw it.

Mr. Christie: Not exactly in the same form.

Mr. Knobel: I read the substance of the paper there.

Mr. Glaisher: It is a new paper, as far as I was aware, or I should not have read it.

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* [Note by Mr. Hind.] Mr. Finlay, First-assistant at the Royal Observatory, Cape of Good Hope, writing on February 24, communicates the following approximate elements of the southern comet, which it will be seen differ widely from those inferred from Mr. Gill's rough observations received by the previous mail:—

Perihelion passage 1880, January 27·55, G.M.T.

	°	'
π	280	16
☊	123	24·5
i	75	12
Log. q	7·90315	

Motion—Direct.

Mr. Finlay doubts if the comet will be now visible, though well above our horizon after sunset.

Mr. Glaisher read a paper from Dr. H. C. Vogel *On the spectrum of Mr. Baxendell's new star in Canis Minor*.

Lord Lindsay said that he had sent out a circular about this star, and that Dr. Copeland had observed its spectrum at Dun Echt, but his observations differed from the observations of Dr. Vogel, in that he had been unable to detect any strong bright lines or marked absorption in the spectrum, although he had used considerable dispersive power, and had examined the spectrum with rock crystal and salt prisms; but nothing peculiar was detected, either at the violet or red ends of the spectrum.

Mr. Glaisher read a letter from Professor C. Wolf, of Paris, referring to a paper by Mr. Maxwell Hall, published in the *Monthly Notices*, *On the Nebula in the Pleiades*, in which he stated that he had not been able to find any recent observations of the nebula. Dr. Wolf referred the society to a review of observations and drawings of the nebula which he had published in the *Annals* of the Paris Observatory for 1877.

Mr. Neison described two papers—one *On the photographic diameter of the moon*, and the other entitled *Determination of personal equation in lunar observations*.

Mr. Dunkin at the January meeting read a paper, in which he asked me for further information as to the method by which I obtained the personal equations given by me in a previous paper. This paper furnishes the information; and it shows that in obtaining those personal equations I had not found it necessary to assume that the personal equations remained constant for a greater period than one year at a time, and it also shows how it is possible to reduce all the observations and personal equations to a uniform standard, even where the observers have been changed. Mr. Dunkin also draws attention to the fact that when he comes to compare personal equations for the two years, 1877 and 1878, with those given by me, he finds them materially different, and therefore, he says, that the explanation I give of the apparent increase of the errors in the mean longitude of the moon is not satisfactory. I would only draw attention to the fact that when you come to compare the fictitious increase which seemed to be existing in 1875 and 1876 with the personal equations, we get a certain value for it, amounting to rather more than one second of arc too great. Now, Mr. Dunkin points out that when you compare the latter observations, you find a diminished value for the personal equations, and, therefore, he does not think these capable of accounting for the apparent fictitious error in the moon's longitude; but at the same time from Mr. Lynn's papers we learn that just in accordance with this great decrease in the personal equations, the apparent error of the moon undergoes a corresponding decrease; a complete confirmation of my views. The apparent decrease in the error of the moon seems to be due to the change of manner in which observations of the moon have been made at the Greenwich observatory.

My second paper is a short notice with regard to a paper by Professor Pritchard. Professor Pritchard claims to have eliminated the systematic errors, because measures from slightly different points by different observers agree. I point out that with Professor Pritchard's determination of the moon's diameter this is not quite the case. He imagines the limb subject to irregularities like the teeth of a circular saw; but in reality the irregularities of the moon are more of the character of long ridges extending for many miles along the limb; so that we should not find much difference from measures taken by the same system. The systematic errors would fail to be eliminated.

Mr. Dunkin: The few figures I inserted in my short paper of January were deduced directly from the tabular errors of the moon determined in 1877 and 1878, and therefore I fully believe in my deductions. The method of observation between 1876, and 1877-8 has perhaps changed, but I cannot see that is any reason why the errors have been increased. Then, again, I am not so certain that the errors of the moon have increased, because when Mr. Lynn, in 1879, discussed the errors deduced from the altazimuth observations, it was found that the observations with the transit circle gave very different results. It is really absurd to attempt to obtain reliable results from so few observations, when you bear in mind that no observer has made more than fourteen or fifteen during the year even under the best circumstances; and I should rather say to Mr. Neison, "Wait a little longer, and get a few more observations before you make any theory on the subject." I know that the value of personal equations do occasionally change from year to year; and I also know that personal equations have so changed in individuals in the last ten years that it is a difficult thing to compare the observations of two observers, one observing in 1865 and the other in 1875. It is rather a difficult matter to get at the truth, because the quantities are so small. It seems a large thing to talk of a second of arc, but it is such a small quantity that to get at the truth you want not fifteen but fifty to obtain a correct result.

Mr. Christie: With reference to the point Mr. Dunkin has referred to, perhaps I may recall a remark of M. Le Verrier, who said, in regard to the present state of observational astronomy, that you want, not to multiply observations, but to multiply observers and observatories. The chief errors in the solar, planetary, and lunar tables come from the personal errors of observers, and the systematic errors in observatories; and we practically depend on a few observatories. With regard to the question of the moon's semi-diameter, it strikes me that Professor Pritchard's method was to take the diameters of the moon at 60 degrees interval, which might be supposed to eliminate these irregularities; but I am not quite clear what the method was, because it has not been explained in detail. There is one point raised as to the value of Hansen's semi-diameter. As a matter of fact, the value that was adopted by Professor Pritchard was adopted by the Astronomer Royal, and on very good grounds, because he took the value of the semi-diameter corresponding to a certain value of the moon's mean parallax, which is a purely conventional thing. It depends on what long period terms you will exclude, and what you will not exclude. Hansen excluded the irregularities due to the effect of Venus. Other theorists have included them. The Astronomer-Royal compared Professor Adams' values of the parallax for a certain period with those used by Hansen. The two sets agreed, I think, to within $\frac{1}{10}$th of a second, so that they were essentially identical; but the values of the mean parallax adopted by Professor Adams and M. Hansen differ by $2''\cdot3$, simply because they attached a different interpretation to the definition of mean parallax. It is not a thing which exists in nature. What exists in nature is the parallax of the moon at a certain time; the mean parallax depends on the calculator and the theory he adopted. The mean diameter must depend on the mean parallax we adopt, and that is a matter of definition. It depends on the lunar theory that is adopted, and no two calculators would agree on that point. If we take Delaunay's we get a different value of the mean parallax to that which we get from Hansen. They might agree for any one year, but they would differ as to what long period terms were to be rejected and what were to be included, in the definition of mean parallax. Supposing they took the same value for the moon's diameter at a given time, they would get different values of the mean diameter, simply because they define the mean parallax differently.

Mr. Neison: When Mr. Dunkin reads my paper, I think he will find all his objections are removed, and that in most cases the personal equations come out with a difference of less than 10 or 12 per cent., showing that they are really nearly constant, and not so variable as he supposes. They may seem variable if you do not adopt, as I have done, a constant standard to which to refer them. With regard to what Mr. Christie says about the parallax, I do not understand it. First of all, he tells us how Hansen arrives at a parallax, but I cannot recollect it, with Hansen's own words, in his tables in which he says he includes all sensible terms of long period, and only excludes the secular inequalities due to the action of the planet upon the eccentricity of the terrestrial orbit. The long period terms due to Venus have practically nothing to do with the parallax. I cannot admit for a moment that we are at liberty to assume somebody else's mean parallax, and then instead of Hansen's tack on to it other results from tables and call it Hansen's parallax. To my mind the only value for Hansen's parallax or for Hansen's semi-diameter are the values given by Hansen in his own table. Again, Mr. Christie says, it is a mere arbitrary value which any theory assigns to the parallax, and that different results would be obtained if we were to adopt Delaunay's theory. As a matter of fact if you take the assumptions which are embodied in Hansen's tables, it does not matter whether you get your theoretical results from Delaunay or whether you take them from Pontécoulant. It has been shown that the value of the lunar parallax will vary by almost infinitesimal quantities.

Lord Lindsay read a paper by Dr. Copeland, entitled *Note on the phenomenon known as "shadow bands."* One evening while the sun was setting, near Dun Echt, Dr. Copeland had observed shadowy bands about 4 in. broad passing over the white wall of the Observatory. A very small section of the sun's disc was just visible at the time over the hill two or three miles distant. Thinking that the observation might throw some light on the shadow bands visible during eclipses, he had watched for the phenomena on other occasions, and had succeeded in observing it several times—once while the sun was still two-thirds visible. Dr. Copeland suggested that the appearance observed during total eclipses was due to the image of the narrow crescent of the sun, the light of which was refracted through denser and rarer masses of air. It is well known that during partial eclipses the round patches of sunlight seen under trees or through small holes become crescent-shaped. Just before the extinction of the sun the patches would become linear, and Dr. Copeland suggested that the difference of the refraction of the atmosphere would become visible with such a linear crescent, when with a broader crescent the images could overlap and extinguish one another. These shadow bands would, he thought, probably be found to move parallel to one another over the country in the direction in which the wind was blowing, and he suggested that the observation referred to in the Eclipse volume of shadow bands, which appeared to move round a centre like the spokes of a wheel, was merely due to perspective.

Mr. Ranyard said that Dr. Copeland's observation of shadow bands upon the Observatory wall, while a large portion of the sun's disc still visible, appeared to him to be a very important fact. There was still a good deal of mystery about this phenomenon. He thought it quite possible that the bands might not be due to the light of the solar crescent, for there were at least four or five observations made by persons who had observed the bands during totality when no solar crescent was visible, and there seemed to be pretty concurrent testimony to the fact that the light of the corona and prominences was not sufficient to cast any shadow. That is, that the light of the corona is overpowered during totality by the light derived from the illuminated clouds on the horizon. The shadow bands are only observed for a few seconds or, at most, two or three minutes before the commencement of totality, when the light of the solar crescent is greatly diminished, and Mr. Ranyard thought it quite possible that the shadowy appearances might be due to light derived from the illuminated clouds on the horizon, which present to the observer the appearance of brilliant sun-set tints. This light would, like the light of the sun observed by Dr. Copeland, have to pass through a long distance of the lower parts of the earth's atmosphere, in which there appears to be much greater variations of density than at higher altitudes. If we do not accept this theory we must assume that just about the time of totality a violent disturbance is set up in the atmosphere, which would cause a very great scintillation of all objects at the sun's altitude as well as on the horizon.

Lord Lindsay: If I recollect rightly Professor Respighi, of Rome, has observed that in the spectrum of a scintillating star there are bands which pass across the spectrum from one end to the other. These might, possibly, have something to do with these bands. It is only when looking at stars low down through varying densities of atmosphere that you find strong scintillation and bands crossing the spectrum.

Mr. Ranyard: The bands you refer to are, I think, absorption bands, which travel through the length of the spectrum. But these shadow bands are seen without a spectroscope.

Mr. Knobel: You refer, I suppose, to the shadow bands described in the Eclipse volume.

Mr. Christie: With regard to scintillation, I think it is explained as caused by total reflection of some portion of the spectrum from the varying densities in different strata of the atmosphere. Some of the rays are reflected and others being differently refracted escape total reflection. It is some time since I looked up the literature of the subject, but I think that was put forward as the explanation by Professor Respighi and generally accepted.

Mr. Glaisher read a paper by Mr. J. Tebbutt *On the longitude of Windsor Observatory, New South Wales,* which had usually been given as 10h. 3m. 15·7s. east, differing by 6 seconds from the value hitherto adopted.

Mr. Green was called upon to read a paper *On some changes in the markings of Mars.* He showed a large map with the configuration of the Martian continents and seas, as laid down from his Madeira observations, and went on to point out some changes which he had noticed during the past opposition. These he compared with the canals and linear markings laid down by Schiaparelli, and he thought that some of Schiaparelli's canals might be explained as being the edges of cloudy areas in the Martian atmosphere, which recur from time to time in the same localities, and are again lost.

Mr. Ranyard described his observations during the total eclipse of 29 July, 1878, made near Denver, Colorado.

He said: I ought to apologise for not having read an account of these observations before, but I thought I had better finish the Eclipse Volume and dispose of other people's observations before I gave an account of my own. In the spring of 1878 I wrote to Prof. Young, asking him to advise me as to the best station within the path of totality to which I could take a large and rather heavy instrument. In answer to my enquiry he was kind enough to invite me to join him as his guest in a camp near Denver. I took with me a camera, with a lens of 13-in. aperture, kindly lent me by Mr. Esdaile. In the first instance I intended to have taken with me an equatorial mounting of the German form; but afterwards I greatly reduced the weight of my packages, by making the box in which the camera was carried form the polar axis of an equatorial mounting, of the description known as the Old English form. The box was about 7-ft. long, and was made with thick elm sides, into which the camera was pivoted at its centre of gravity, so that when the top and bottom of the box were removed, and the box was set up upon a wooden framework, so as to form a polar axis, the camera could be moved equatorially, and the box was driven by means of the clockwork motion of an ordinary equatorial mounting so as to follow the sun. On the top of the large camera was a small spectroscopic camera with a single prism of 60°, lent me by Prof. Young, but no slit. With this an exposure was made just before the commencement of totality in the hope that a series of images of the lower chromosphere, frequently spoken of as Young's bright-line stratum, might be obtained, but although only one prism of 60° was used, no trace of the spectrum of Young's stratum is to be seen upon the plate. The same plate was exposed during the whole of the totality, and there is an image of the continuous spectrum of the corona which extends from a little below G for about one-fourth of the length of the spectrum of the solar crescent visible after totality, but no separate images of the corona are visible corresponding to the bright lines, which have been observed in the spectrum of the corona. With the larger camera two photographs of the corona were obtained, but owing to an accident by which the camera was pushed from off the sun's place, no further photographs were obtained with it.

It was necessary, in order to make the exposures with the larger camera, to call in the aid of four assistants. One of these put the plates into the camera, and removed them after the exposures; another removed and replaced a screen in front of the camera to make the exposures, and a third counted time, while a boy handed the slides containing the plates to the first assistant. Unfortunately, owing to an accident, the camera received a push during totality, and we only obtained two photographs with it, the sun having been pushed off the plate between the second and third exposures. While the friends who had volunteered to assist me were making the exposures with the large camera, I was observing the polarization of the corona, with a small 3-in. telescope and a biquartz. I found that the polarization of the corona could be detected by strongly-marked colours quite down to the moon's limb, both in the area of the polar rifts and within the brighter equatorial structure of the corona, and I succeeded in tracing the radial polarization of the corona to the distance of a solar diameter and a half from the moon's limb, in the eastern equatorial regions.

The corona observed during this eclipse belonged to what I have called in the Eclipse Volume the sun-spot minimum type of corona. That is, there was a great extension of the corona in the equatorial regions, but very little in the neighbourhood of the sun's poles. None of the groups of synclinal structure which have been observed in coronas seen at times when sun-spots are more developed, were observed in this corona. According to Dr. Wolf, the relative sun-spot number for July, 1878, was 3·3, which is very small.

In reply to Capt. Abney,

Mr. Ranyard said the lens he used was 13-in. aperture. It was one made by Ross some time ago. The ratio of the aperture to the focal length was about 1 to 6. There was no diaphragm, so the whole pencil was made use of. The reason he had not made use of a reflecting telescope, which could be made with a still greater proportionate aperture, was that the scattered light of the prominences and lower parts of the corona might have interfered with the fainter parts of the corona. Lord Lindsay's photographs taken in 1870 were fogged, he thought, from this cause. The plates were dry plates by Messrs. Mawson and Swan.

Mr. Christie said he had a paper *On the mean heliographic latitude of sun-spots deduced from the observations taken at Greenwich.*

Mr. Christie added that they had been taking lately at Greenwich some photographs of the spectra of sun-spots, and had enlarged them. They were in the nature of an experiment, but would be perhaps interesting to the Fellows to see.

Mr. Brett said the Society was invited two months ago by Mr. Marth to watch the comparative brightness of Mars and Aldebaran during February. Up to the 28th of February he observed that Mars had decidedly the predominance in brightness, but on the 4th of March that appearance was reversed, though it was not possible to compare them very accurately on that occasion, Aldebaran appearing to be the brighter, and afterwards he considered they were of equal brightness.

Lord Lindsay said this matter had been watched at his observatory, and the report was that up to the 10th of March Mars still remained brighter than Aldebaran.

Mr. Chambers asked what had become of a paper by Mr Burnham, criticising Admiral Smyth's double-star measures in the cycle, which he understood had been presented to the Society.

The President stated that the paper was under the consideration of the Council, and he could not venture to say whether the Council would determine to print it or not.

The meeting adjourned at ten o'clock.

REVIEWS.

Meteorological Observations made at the Adelaide Observatory during the years 1876 *and* 1877, under the direction of Charles Todd, C.M.G., F.R.A.S. Published by authority of the Government of South Australia. Adelaide. 1878. Pp. 133.

Besides most complete and admirable meteorological observations, this volume contains observations on the phenomena of Jupiter's satellites, made with the 8-in. equatorial (by Cooke). Mr. Todd is not only observer, but Postmaster-General and Superintendent of Telegraphs.

Annuaire de l'Observatoire Royal de Bruxelles. 1879. 46e Année. Bruxelles. F. Hayez. 1878. Pp. 373.

The first part to page 149 is occupied with the ordinary useful matter, and the remainder contains amongst others the following interesting papers:—Great periods in the movements of the stars, by J. C. Houzeau (the Editor); Comets, by C. Pilloy; On the earthquake of August 26th, 1878, by A. Lancaster; Astronomy at the Paris Exhibition, by L. Estourgies; Bibliography of works, memoirs and notices on spectroscopy which have an astronomical interest, by Ch. Fievez (also printed separately); Discourses pronounced over the tomb of M. Ernest Quetelet, by MM. Mailly and Hooreman. M. Quetelet had been unceasingly occupied for above twenty years in the laborious preparation of an extensive catalogue of small stars with proper motions, which his lamented decease prevented his completing; but his colleagues in the observatory have undertaken this, so that we may anticipate the appearance of this great work before long.

Passage de Mercure devant le soleil, le 6 *mai* 1878, par Ad. de Boë, conseiller (Conférence donnée 14 juillet 1878). Deuxième édition. Extrait des Bulletins de la Société de Géographie d'Anvers. Anvers 1879. Pp. 36 and diagrams.

M. Ad. de Boë has treated of the transit of Mercury in a very clear manner, and has besides managed to embody in his pamphlet a considerable amount of astronomical information. He is inclined to assign the appearance of the luminous point seen on the planet to electrical phenomena of greater intensity than any polar auroras in our own globe.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.
To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE TRAPEZIUM IN ORION.

Sir,—I have read with great pleasure Mr. Gledhill's very interesting letter in this month's *Astronomical Register*, on that remarkable asterism θ^1 Orionis; and would heartily endorse his request, or recommendation, that it should be carefully observed, with a view to the solution of the question as to the variability of its components. Unfortunately, Orion is now getting rapidly towards the west at a tolerably early hour in the evening; and the perfectly frightful weather with which astronomers have been afflicted during the last five weeks has effectually precluded (as its continuation seems destined to preclude) anything like sequent observation. I am afraid then that systematic study of the noteworthy object to which Mr. Gledhill has invited our attention will have to be postponed until next winter.

Meanwhile, it may not be wholly uninteresting if I give a few extracts from my own very desultory observations of this curious multiple star. Beginning then with 1857, I find that while the fifth star was steadily seen in the autumn, it got fainter towards the end of the year; although, at the beginning of the succeeding year (1858) it was visible in bright moonlight. In January, 1859, I saw it in an illuminated field; but in the November of that year it had become much fainter. It was very bright and conspicuous in December, 1863. Going now, *per saltum*, to January, 1874, I find a note in my observatory book, that the sixth star was almost—if not quite—as bright as the fifth. This was on the 21st. On the 25th, only four nights afterwards, while the fifth was quite bright, I could not see the sixth at all! The fifth star was fainter again early in 1875; but at the beginning of 1876 resumed its brightness. At its second apparition at the end of that year, I made a note (on December 22nd) that both stars were very well seen; the sixth *rather* the brighter of the two. The last memorandum I can find upon the subject bears the date of February 25th, 1878, when I seem to have noted that the fifth star was steadily seen, and the sixth only glimpsed at intervals.

Disconnected as these observations are, and valueless as they may be for the purpose of determining the period of variability of the fifth and sixth stars in the trapezium, they offer to my mind very cogent evidence indeed that such variability exists. Hence I can only express a hope that Mr. Gledhill's communication will attract that attention which it undoubtedly merits.

I am, sir, most obediently yours,
WILLIAM NOBLE.

Forest Lodge,
Maresfield, Uckfield:
March 6, 1880.

THE YOSEMITE VALLEY AND LUNAR RILLS.

Sir,—The lunar rills are so unlike anything to be seen in our part of the world that perhaps the following description of the Yosemité may be acceptable to your readers. It is taken from Player-Frowd's *Six months in California*.

"The Yosemité proper is seven miles long. It can hardly be called a valley. It is in reality a rift in the earth's surface. Let the reader imagine such a chasm, of a width varying from one mile to ninety feet, with granite walls from one thousand to four thousand feet; that is, from one-fifth to three-quarters of a mile high. Let him imagine some of these masses of rock detached and standing in their solitude, like giant obelisks. Let him picture others clefts from top to bottom, as if by a thunder-bolt. Added to this, let him imagine a river cold as ice, clear as crystal, following the windings of the valley, that same river having descended as from the clouds, with the thunder of a great flood. Let him conceive the most luxurious vegetation and the extreme of barrenness, the softest carpet of moss and grassy lawns, great ferns and wild roses, alternating with the huge scathed rocks, where not even the lichen will cling, and then he will have a prosaic idea of the valley of the Yosemité ..... Here are spots where the sun never shines, cold, damp, and dripping for ever, and others where the gorge opens its arms wide to receive the 'bridegroom' and bask in his rays; some where the river hurries along, anxious to be free from its stony prison, and others where it expands into a still, deep lake, as if for rest, and enjoyment of the lovely scene, for it takes it all in, and in its depths the whole valley is mirrorred."

I am, sir, your obedient servant,
W. J. B. RICHARDS.

Bayswater: March 5, 1880.

THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—Allow me to say one word with respect to Mr. Chambers' letter in the February number of the *Astronomical Register*. Although I am no advocate for making any sweeping changes in the bye-laws of the Society, yet I think it would only be an act of justice to those Fellows who reside at a considerable distance from the metropolis, that they should be permitted to have a proxy vote. The elections are now virtually in the hands of a minority, who happen to reside in or near London. I can see no reasonable objection to such a plan, and then the Council could not fail to be a thoroughly representative body.

Yours faithfully,
W. S. FRANKS.

Leicester: March 2.

JUPITER'S ROTATION PERIOD.

Sir,—In your report of the January meeting of the Royal Astronomical Society, there is a departure from your usual accuracy. On page 26 you have been good enough to mention my paper on the above subject as being read at the meeting. But, unfortunately, you give as my determination of the rotation period a value which is certainly not mine, nor anything like it. Therefore, I must ask you to publish this in order to correct a false impression. The value of the rotation period arrived at in my paper was 9h. 55m. 33·91s. of mean solar time.

Yours truly,
HENRY PRATT.

JUPITER.

Sir,—On the 6th December, 1879, I observed the planet Jupiter with my 7⅓-inch refractor under pretty favourable circumstances, and obtained a few rough measures which may perhaps be of interest for comparison with those of Mr. Gledhill, printed in the March number of the *Register*.

The red spot was of a clear ruddy or ruddy-brown colour. Its centre was estimated to be on the central meridian at about 6h. 45m. G.M.T. The southern portion of the equatorial belt was of a decided grey tint, and its lower edge was broken up into a series of festoons, or rather arches with the concavities facing north. The lower or northern portion of the belt was of a ruddy-brown hue, and was divided in two throughout its length by a narrow white line.

With a parallel-wire micrometer, and mag. power 330, a few measures were taken with the following results:—the distances being measured from the lower or northern edge of the disc.

Northern margin of disc to lower edge of equatorial belt, 15″.
Upper edge of equatorial belt, 21″.
Centre of red spot, 27″.
Southern edge of disc, 40″.
Length of red spot, 13″; breadth, 3″.

The equatorial diameter of the planet, as measured was about 42″. Setting the webs about half this distance apart, and keeping one of the webs on the preceding limb, the *following* extremity of the red spot was judged to be on the central meridian at 7h. 15m. G.M.T.

I am, sir, yours faithfully,
GEORGE KNOTT.

Knowles Lodge, Cuckfield:
March 13, 1880.

ON THE STARS VISIBLE TO THE NAKED EYE.

Sir,—The number of stars visible to the naked eye is often much underrated. I have mapped all the stars visible to my naked eye within 5° of the northern half of the ecliptic, and it may be interesting to show how they are distributed. The following table gives (A) the number of stars in my maps for every 5° of longitude; (B) the number in R. A. Proctor's Star Atlas, down to the sixth magnitude (likewise between lat. + 5° and —5°); (C) the number in his Atlas which are invisible to my naked eye;

(D) the percentage of the stars in my maps, as compared with his, calculated for every 15° of longitude. At the foot are the results for each 45° of longitude.

Longitude. A.D. 1880. ° °	A	B	C	D
0— 5	6	2		175
5— 10	6	4	1	
10— 15	9	6	1	
15— 20	8	5		200
20— 25	9	4		
25— 30	5	2		
30— 35	8	2		227
35— 40	7	4		
40— 45	10	5		
45— 50	9	7	1	181
50— 55	11	3		
55— 60	18	11	2	
60— 65	19	8	1	215
65— 70	15	8	1	
70— 75	9	4		
75— 80	11	6		214
80— 85	18	6		
85— 90	18	10		
90— 95	17	8	2	265
95—100	14	4		
100—105	14	5		
105—110	11	4		253
110—115	16	5		
115—120	16	8		
120—125	15	8	1	233
125—130	12	3	1	
130—135	11	4		
135—140	13	3		222
140—145	10	5	1	
145—150	17	10	1	
150—155	15	9	1	218
155—160	12	4	1	
160—165	10	4		
165—170	20	8		360
170—175	14	4		
175—180	15	2		
Total	448	195	15	230

Longitude. A.D. 1880. ° °	A	B	C	D
0— 45	68	34	2	200
45— 90	128	63	5	203
90—135	126	49	4	257
135—180	126	49	4	257

This table shows no perceptible diminution in the clearness of the atmosphere at the lowest part of the ecliptic examined (which has a meridian altitude here of 35°) as compared with the highest part (meridian altitude 58°), unless this diminution in clearness affects Proctor's atlas.

Proctor's and my maps agree in showing the great increase in the density of the stars in the Milky Way (long. 55° to 125°); the remarkable gap therein between the Hyades and the main stream and the sparseness of stars in Pisces and Aries; but the rich region so striking in my maps, between long. 165° and 180°, does not appear in Proctor's.

To estimate the number of stars visible to the naked eye in each hemisphere, let us suppose them to be strewn with stars in the same proportion as the portion I have mapped; this would give in each 5,140 visible to my naked eye, and 2,237 in Proctor's maps down to the sixth magnitude. But it would appear that this portion is rather less rich than the average of the northern hemisphere, for though Proctor does not state exactly the number of such stars he has given in this hemisphere, he says it is rather less than 2,487, while in the whole sphere he has given 5,932. If we may increase these numbers in the same ratio as the region I have mapped, we shall obtain, as the number of stars visible to the naked eye, about 5,630 in the northern hemisphere and about 8,000 in the southern, or 13,630 in the whole sky. A recent writer, has, however, thrown doubt on the correctness of Proctor's supposition of the excessive density of the stars in the southern portion of the southern hemisphere; on this point I do not express any opinion.

Many persons, on looking at the sky, would say that the number of stars they could see was far greater than these I have given; I think this partly arises from the twinkling of the stars, and from the uncertainty of vision, which seems to cause numbers of minute points to appear and disappear—"glimpse objects," as astronomers call them—most of which have probably a real existence, though seen too momentarily for the mind to be certain of them, while the others are only imagined, but yet give the effect of numbers to the mind. In my maps I have not inserted any stars but those I could see without doubt, though many were only glimpsed, and not seen steadily. A further cause of the great apparent number of the stars is the Milky Way, a part of which is always above the horizon, and though the naked eye cannot resolve it into stars, it can perceive its granular texture betokening resolvability, and it thus gives the impression of numbers of stars.

I might state that I am slightly short-sighted, the effect of which is to render invisible and to prevent my separating close stars which can be seen separate by many.

Sunderland: Feb. 25, 1880. T. W. BACKHOUSE.

LESSONS IN SELENOGRAPHY.

By the Rev. W. J. B. Richards, F.R.A.S., and W. R. Birt, F.R.A.S.

Chapter II.

Section I.—During the nightly scrutiny of the moon's surface in the course of one lunation, the tyro would not fail to remark a variety of formations manifested by differences of light and shade on the moon's disk. Commencing his observations again between two and three days of the moon's age, he may give a prolonged attention to the somewhat large object near the western limb, and notice some of its peculiarities. It is a very characteristic type of the smaller *Maria*, and, as we said in Chapter I., it is known by the appellation of *Mare Crisium*, or Sea of Crises. The student may first direct his attention to the mountain border, especially on the east, which he will find characterised by lofty mountain peaks, casting long shadows when the terminator is passing over the *Mare*. This mountain border is marked by a depression which both on the north and south descends even to the level of the surface of the *Mare*, forming a "pass," which allows the interior surface to communicate with a portion enclosed by mountains apparently at the same level. When illumination and libration are favourable, the two promontories north and south of the pass are seen to have, just on the extremity of each, a craterlet; as both these objects are sometimes *hidden* by libration, the record of each observation of the border and of the *Mare* should contain a notice of the appearance or otherwise of one or both of them, nor should this be forgotten although there should be no prospect of *publication*. A journal well kept and deposited in safe hands for future preservation will always be useful, and may be very valuable. The north and south borders of the *Mare* are, as well as the east, characterized by high mountains, but on the west the mountain border (proper) is absent, the surface of the *Mare* being traceable (shortly after full moon) among apparently isolated mountains, forming an interesting contrast with the eastern border. Every peculiarity noticed in the mountains surrounding the *Mare Crisium* should be particularly described.

Another class of features characterising this and several of the other *Maria* is the occurrence of several ridges upon the surface, different from *mountain* ridges, but rather analogous to the bergs running parallel with the course of a terrestrial river; they may be described as *soft* and *undulating*, as distinguishing them from mountain ridges broken into separate peaks and appearing hard to the eye. The soft ridges generally preserve a marked parallelism to the borders, but those which pass longitudinally through the centres of the *Maria* are mostly straight. This arrangement suggests the welling-up of a material in a liquid or semi-liquid state, through a longitudinal fissure which has spread out east and west in shallow waves, the ridges marking the crests of the waves at the time of consolidation, somewhat as sheets of basalt have spread out on the earth's surface.

The craterologies of the *Maria*, especially of the Mare Crisium, demand attention. There are at least 49 craters, including the three larger craters, Picard, Peirce, and Peirce A, on its surface. In the report of the Committee of the British Association, appointed at Bath for mapping the surface of the moon, and presented at Birmingham in 1865, pp. 292, 293, will be found a table of 49, with the names of the discoverers and the dates of discovery. It is very necessary that the surface should be most carefully examined first, to ascertain how many of these 49 are still in existence, how many new ones have broken out during the last 14 years, and a record made of their relative brilliancy and sizes. It would be well to institute such an examination periodically, at least every six months. We are not aware that any attempt of this kind has been made since 1865.

In the neighbourhood of the *Mare Crisium*, on the east, is a distinct individualized formation known as the *Palus Somnii*, or Marsh of Sleep. It has, however, more the appearance of a raised portion of land. Webb ("Celestial Objects") describes it as "an uneven, always distinguishable surface, has a peculiar tint, perhaps yellowish brown, unlike the simple grey of the *Mare Tranquillitatis.*" As the sun declines upon it, the edges towards the S.W. and N.W. respectively become very brilliant. Near it is the bright crater Proclus, of which Webb says, "it is the centre of several light streaks not very easily seen." During the year 1879, these streaks have been very assiduously observed, and we hope that in due time the results will be given to the public. So far as the examination of the observations has yet proceeded, it would appear that the streaks do not radiate from *Proclus* itself, but from two or three points near it, and that in some cases the streaks *intersect* one another. The real nature of the streaks has not yet been determined. Three of them extend nearly across the *Mare Crisium.*

There are several distinct classes of formations on the moon's surface which it may be well for the student to make acquaintance with, they may be classified thus :

Classes of Formations.

I. 1 Mare, or Sea.
  2 Palus, or Marsh.
  3 Lacus, or Lake.
  4 Sinus, or Bay.
II. Bright Plains.
III. Mountains.
  1 Isolated.
  2 Ranges.
  3 Ridges.
IV. 1 Walled Plains.
  2 Mountain Rings.
  3 Crater—Plains.
  4 Craters—steep walls, 4 to 10 miles in diameter.
  5 Craterlets--small, with steep walls, from ½ mile to 5 miles.
  6 Crater Pits—very shallow.
  7 Depressions—shallow without any wall.
V. 1 Rills.
  2 Crater-rills.
  3 Crater-rows.
  4 Faults.

(*To be continued.*)

ASTRONOMICAL OCCURRENCES FOR APRIL, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. β Leonis
Thur	1	18 12	☾ Moon's Last Quarter			11 0·2
Fri	2		Sidereal Time at Mean Noon 2h. 43m. 6·23s.			10 56·3
Sat	3		Sun's Meridian Passage 3m. 17·92s. before Mean Noon			10 52·4
Sun	4		Saturn's Ring: Major axis=36″·17 Minor axis= 7″·37	Jupiter's satellites invisible through nearness to the Sun.		10 48·5
Mon	5					10 44·5
Tues	6					10 40·6
Wed	7	4 17 20 20 22	Conjunction of Moon and Venus 6° 50′ S. Conjunction of Saturn and Sun Conjunction of Moon and Mercury 5° 0′ S. Conjunction of Moon and Jupiter 6° 36′ S. Conjunction of Jupiter and Mercury 1° 35′ N.			10 36·7
Thur	8					10 32·7
Fri	9	3 7 6	● New Moon Conjunction of Moon and Saturn 7° 43′ S.			10 28·8
Sat	10					10 24·9
Sun	11	7 27 8 20	Occultation of 47 Arietis (6) Reappearance of ditto			10 20·9
Mon	12					10 17·0
Tues	13					10 13·1
Wed	14	8 38 9 33 18	Occultation of 121 Tauri (6) Reappearance of ditto Conjunction of Venus and Mercury 0° 19′ N.			10 9·1
Thur	15	3 14	Conjunction of Moon and Mars 1° 5′ N. Conjunction of Jupiter and Venus 0° 30′ S. Illuminated portion of disc of Venus=0·916 Illuminated portion of disc of Mars=0·915			10 5·2

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. β Leonis —
Fri	16	10 43 11 25 12 48 13 40	Occultation of 56 Geminorum (5½) Reappearance of ditto Occultation of 61 Geminorum (6) Reappearance of ditto	3rd Sh. I.	16 59	10 1·3
Sat	17	7 14 10 58 21	☽ Moon's First Quarter Near approach of ζ¹ Cancri (4½) Conjunction of Jupiter and Mercury 0° 48′ S.			9 57·3
Sun	18		Sidereal Time at Mean Noon 3h. 46m. 11·13s.			9 53·4
Mon	19		Sun's Meridian Passage 3m. 42·17s. before Mean Noon			9 49·5
Tues	20	14 56	Near approach of 36 Sextantis			9 45·5
Wed	21	12 10 13 14	Occultation of ε Leonis (5) Reappearance of ditto			9 41·6
Thur	22	11 25	Near approach of 14 Virginis (6½)			9 37·7
Fri	23			2nd Tr. I.	16 14	9 33·7
Sat	24	10 50	○ Full Moon Saturn's Ring: Major axis = 36″·28 Minor axis = 8″·03			9 29·8
Sun	25					9 25·9
Mon	26	15 58	Near approach of 22 Scorpii (5)			9 21·9
Tues	27					9 18·0
Wed	28	12 33 16 20 16 57	Near approach of 24 Sagittarii (6) Occultation of 26 Sagittarii (6) Reappearance of ditto			9 14·1
Thur	29					9 10·1
Fri	30					9 6·2
MAY Sat	1	3 7	Conjunction of Saturn and Venus 0° 47′ N. Conjunction of Neptune and Sun			9 2·3

THE PLANETS FOR APRIL.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	0 15 32	N. 3 48½	11″·3	23 30·8
	9th	0 5 52	N. 0 27½	10″·4	22 49·7
	17th	0 15 13	S. 0 21	9″·2	22 27·6
	25th	0 39 35	N. 1 16	7″·9	22 20·4
Venus ...	1st	23 11 18	S. 6 40½	11″·4	22 26·7
	9th	23 47 40	S. 2 57½	11″·1	22 31·5
	17th	0 23 44	N. 0 51½	10″·8	22 36·1
	25th	0 59 49	N. 4 41	10″·6	22 40·6
Mars ...	1st	5 35 33	N.25 5½	6″·8	4 53·9
	9th	5 55 21	N.25 10	6″·5	4 42·1
	17th	6 15 26	N.25 5	6″·2	4 30·7
	25th	6 35 41	N.24 50	6″·0	4 19·5
Uranus ...	2nd	10 30 28	N.10 14½	4″·2	9 44·0
	18th	10 28 53	N.10 23½	4″·2	8 39·6

Mercury rises just before the sun at the beginning of the month, the interval increasing to about half an hour before sunrise.

Venus rises about half an hour before the sun throughout the month.

Mars sets about an hour and a half after midnight at the beginning of the month, the interval decreasing.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN APRIL, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Picard, Peirce, and Peirce A. We have selected these objects for observation during the spring months of 1880, inasmuch as they are surrounded by an appendage of which we do not remember that any previous account has been given. Each of these craters is surrounded by a dark aureola, which extends some little distance from the walls. The colour of these aureola is decidedly darker than the adjoining surfaces of the *Mare*, and in the present state of our selenographical knowledge difficult of explanation. Perhaps without some terrestrial analogy their origin may ever remain unknown to us; but we venture to suggest a perusal of *Scrope's Geology and extinct volcanos of Central France*, particularly chapters v. to viii. It has generally been considered that every appearance on the moon's surface of a volcanic character indicates a state of quiescence, and that the volcanos once active are now extinct: if, therefore, the moon be considered as a region of extinct volcanos, a similar region on the earth is perhaps the best for comparison, such a region is found in Central France, and is described as the granitic platform of

Auvergne, its length is twenty miles by two wide, it attains an elevation of 2,800 feet above the sea, and from this, otherwise level plain or platform, rises a chain of volcanic cones, or *Puys* as they are locally called, which with the scoriæ and ashes vomited by the cones when in eruption form a notched and irregular ridge running north and south.

The most remarkable feature connected with the granitic region is, that it is fringed both on the east and west by extensive beds of basalt of a *dark colour,* which escaping from the bases of the cones when in a state of eruption, flowed down the more or less precipitous slopes of the elevated platform, and spread over a portion of the low lands bordering the rivers Allin and Sioule respectively. It is this basaltic fringe to which we desire to solicit the attention of the selenographical student, especially as a terrestrial analogue of the dark aureola of Picard, Peirce, and Peirce A; and in connection with that of Peirce A, we would remark that during the year 1879 it has been seen partly surrounded by a light rim, to which a light streak extended from a point S.W. of Proclus, presenting the appearance of a hook. We should recommend the following micrometer observations of the three craters; *first,* the longest and shortest diameters of each *between the walls. Second,* the extent of aureole in the same direction from the *exterior* of the walls; and, *third,* the measurement of the chord of the visible segment of the bright ring in immediate contiguity with the aureole of Peirce A.

In a communication to the *Observatory,* for March, Mr. W. F. Denning, on "the faint stars in the Trapezium of Orion," mention is made of some observations of the late Mr. Edmund Salter, of Manchester, in which the following passage occurs:—"As a proof of the excellence of Mr. Salter's telescope (a fine 12-inch mirror, by Bird), it may be mentioned that on January 15, 1870, he saw nineteen spots or craterlets upon the floor of the lunar crater Plato." We shall be glad to receive accounts of observations of the floor of Plato, made with telescopes more than six inches aperture, addressed to—

Water Lane, Stratford, E.

SATELLITES OF URANUS.

Major and minor semi-axes *a* and *b* of the apparent ellipses described by the satellites, and position-angles p_0 of the major axes:

	Ariel.		Umbriel.		Titania.		Oberon.		
1880.	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	p_0
	"	"	"	"	"	"	"	"	°
April 4	15.10	1.68	21.04	3.73	34.51	6.12	46.14	8.19	13.47
14	15.00	1.73	20.90	3.81	34.28	6.24	45.84	8.35	13.42
24	14.89	1.76	20.74	3.85	34.02	6.31	45.49	8.44	13.38

The satellites, moving in the direction of decreasing position angles will be at their greatest northern and southern elongations N. and S. (in position angle 13°.4 and 193°.4), and at their inferior and superior conjunctions (in pos. 283° and 103°) about the following hours, G.M.T.:

Ariel.		Umbriel.		Titania.		Oberon.	
1880.	h.		h.		h.		h.
April 2	5·4 N.	April 1	18·3 S.]	April 2	21·5 sup.	April 0	23·2 N.
3	11·7 S.	3	20·0 N.	5	1·7 N.	4	8·0 inf.
4	17·9 N.	5	21·8 S.	7	6·0 inf.	7	16·9 S.
6	0·2 S.	7	23·5 N.	9	10·2 S.	11	1·7 sup.
7	6·4 N.	10	1·3 S.	11	14·5 sup.	14	10·5 N.
8	12·7 S.	12	3·0 N.	13	18·7 N.	17	19·3 inf.
9	18·9 N.	14	4·7 S.	15	23·0 inf.	21	4·1 S.
11	1·2 S.	16	6·5 N.	18	3·2 S.	24	12·9 sup.
12	7·4 N.	18	8·2 S.	20	7·4 sup.	27	21·7 N.
13	13·7 S.	20	9·9 N.	22	11·7 N.	May 1	6·5 inf.
14	19·9 N.	22	11·7 S.	24	15·9 inf.	Ariel.	
16	2·2 S.	24	13·4 N.	26	20·2 S.	Apl. 24	21·9 N.
17	8·4 N.	26	15·1 S.	29	0·4 sup.	26	4·1 S.
18	14·6 S.	28	16·9 N.	May 1	4·7 N.	27	10·4 N.
19	20·9 N.	30	18·6 S.			28	16·6 S.
21	3·1 S.					29	22·9 N.
22	9·4 N.					May 1	5·1 S.
23	15·6 S.						

A. M.

Erratum on p. 67, fourteenth line from bottom, for *Major* read *Minor*.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1879.

Dobie, Dr.
Green, N. E.

To Dec., 1880.

Fielding, G. E.
Gore, J. E.
Herchel, Major J.
Parnell, J.
Terby, Dr. F.

To April, 1881.

Fry, W. E.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to John C. Jackson, at Lower Clapton, London, E.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 209. MAY. 1880.

ROYAL ASTRONOMICAL SOCIETY.

Session 1880—81.

The second Meeting of the Session was held at the Society's rooms, Burlington House, Piccadilly, on Friday, April the 9th.

E. Dunkin, Esq., F.R.S., *Vice-President*, in the Chair.

Secretaries—W. H. M. Christie, Esq., and J. W. L. Glaisher, Esq.

The minutes of the March meeting were read and confirmed.

Lieut. T. Preston Battersby, R.A., of Cromlyn, Rathowen, Westmeath, Ireland,

was duly elected a Fellow of the Society.

Mr. Christie announced that 74 presents had been received since the last meeting of the Society. Amongst these was a cometarium presented to the Society by Mr. Jackson Gwilt, and a folio book presented to the Society by Mr. Knobel, entitled "De astro specula," published in Vienna. The thanks of the Society having been voted to the donors,

Mr. Chambers said: Will you kindly oblige me, sir, with an official answer as to the decision of the Council on the requisition which I handed in as to the proposed alteration of the Bye-laws?

The Chairman: The requisition was laid, as far as I recollect, before the Council at the meeting in March last, and the Council by a majority declined to proceed. A motion was proposed by the Secretary, that the question be referred to a committee, and that motion was lost.

Mr. Chambers: Then I take it that, as the answer is not satisfactory to the five Fellows who signed the requisition, I may further ask, Is it competent for me to give notice for a special meeting to consider the question in June, or what will be a proper step under the Bye-laws, they are not very clear?

The Chairman read the Bye-law providing that any five Fellows might recommend an alteration in the Bye-laws, by letter to the Secretary, and that the Council should take such recommendation into their consideration at the next meeting, and if their decision was not satisfactory to the requisitionists, the Council should bring the subject before a special general meeting of the Society which should be held within a month.

Mr. Chambers called attention to the 55th Bye-law, which seemed to contradict the other, as it required a month's notice to be given to the Council of any proposal to alter a Bye-law.

The Chairman: The Bye-laws do seem to want revision on many points.

Mr. Chambers inquired whether notice given five weeks before the June meeting would be sufficient?

The Chairman thought it would be, but reminded Mr. Chambers that five Fellows must sign the requisition.

Mr. Neison thought the notice calling the meeting ought to specify the exact alterations which were to be proposed in the Bye-laws. As he understood, the requisition to the Council recommended the revision of the Bye-laws, but did not say in what respect.

Mr. Barrow observed that the question of the interpretation of the Bye-laws that had been read seemed to him sufficient evidence that some alteration in the Bye-laws was needed.

Mr. Chambers thought the requisition to the Council was clear enough. It requested a revision of the Bye-laws. If others would join him he would prepare a requisition in the terms of the Bye-laws that had been read.

The Chairman read the names of several gentlemen in default in regard to their subscriptions, and declared them to be "expelled the Society."

Mr. Christie read a note by Mr. R. H. M. Bosanquet, *On a precessional globe,* which he had made.

Mr. Christie read a paper by Dr. Konkoly *On* 410 *radiants of shooting stars deduced from observations made in Hungary in the years* 1871 *to* 1878, *and* 80 *probable radiants deduced from the* 410 *radiants.* The 410 radiants were founded on 2998 observations of meteorolites, but only 1600 of them were used to determine the radiants.

Another paper on the same subject was read from Dr. W. Barker, second officer of the "Superb." These meteor observations were made on a voyage between London and Melbourne.

The Chairman called on the Astronomer-Royal to read a paper *On the theoretical value of the acceleration of the moon's mean motion in longitude, produced by change of eccentricity in the earth's orbit.*

Sir George Airy said: Perhaps, before I make any allusion to the paper, I may advert to two matters which have no connection at all with it. At the anniversary meeting of the Society I was prevented by illness from attending, or I should have been glad to advert to the account of the Eclipse Observations published by the Society, under the care of Mr. Ranyard, and to have said it does honour to the Society as well as to Mr. Ranyard. (Applause.) The next thing I have to say is, that I have had the great advantage this afternoon of seeing the present which has been made by a member of the Council, of a book on the state of the instruments in the Vienna Observatory in the reign of Maria Theresa. The book is valuable as recording the condition of the instruments used at the particular period of time when the great change of modern astronomy telescopes was just beginning to come about, and when the excellent clockwork by Graham was beginning to be used for accurately determining right ascension, instead of determining it by the distance of one star from another all round the sphere.

On the subject of his paper, the Astronomer-Royal said: In the *Monthly Notices* of the Royal Astronomical Society for January, 1874, I explained a new method of treating the lunar theory. I dispensed with long algebraical expressions, and used the numerical co-efficients deduced by other investigators—Delaunay and others. The immediate object was to find the numerical relation between the theoretical movements of the moon and the forces on which those movements depend, and which ought to account for these movements; and to find whether by modifying the movements, and the forces depending on those modifications, the relation between the movements and forces can be made perfect. That was the original intention; but it is evident that this process is not limited to the correction of assumed co-efficients, but can be applied to the examination of the effect of introducing small forces not contemplated in the original theory. These small forces may depend on geometrical considerations in some cases, and on time in other cases. In such cases any changes from the original supposition are, in fact, new forces; and here we have to consider how, by varying the movements correspondent herewith, the relation (of the forces and movements) may be made perfect.

If we suppose the sun's action upon the moon's motion to be diminished or increased slightly, that may be represented by considering the original forces to be maintained and complementary forces to be introduced, which will represent the amount of diminution or increase, as the case may be. (The Astronomer-Royal here gave a technical description of the means by which he had determined by this method the value of the secular acceleration of the moon's motion.) He remarked, the result is that thus I obtained finally for the variation for a single year $\frac{1}{1000}$th part of a second, and for a century 10 seconds and a fraction.

I present this result to the Society with much confidence. It is to be remarked that this numerical value is founded on the numerical value attached to the solar parallax, which has been taken in the first place as 8″·9; and depends upon the inverse cube of the sun's distance, or upon the cube of the parallax. I take leave to remark that this is the toughest investigation I ever had in my life. It has occupied me, in one shape or another, for many years, sometimes attacking it and sometimes repelled by it for months together. It is the most slippery thing I ever laid my hand upon, but I am quite sure I have a firm grasp on it at last, and if I had the least doubt of the perfect accuracy of it I would not lay it before the Society. I need not say it is a matter of great importance, in matters in which I have been formerly engaged, especially in distant chronology. The total solar eclipses of former times depend entirely upon this small quantity, and I think that in this investigation, which has cost me so much pains, I have worthily bestowed some trouble to give a little certainty to that point.

Mr. Stone (in reply to the Chairman's call): The only remark I have to make is this: Years ago I had to work out this question for my own satisfaction. I obtained the same result as Professor Adams had obtained. As the Astronomer-Royal says, it is a very slippery subject indeed. Of course it is impossible to follow the details of the Astronomer-Royal's work from hearing it read, but I am quite sure of one point, that we shall all be agreed in our admiration of the energy and skill the Astronomer-Royal has displayed in this investigation. He began life by theoretical investigations of the planetary theories, making one of the most important discoveries ever made, and now towards the close of his life we see him engaged in a great investigation, and carrying it, we hope, to a successful conclusion almost now. Really, I cannot find words to express my admiration of the course that he has adopted, and I can only hope that he may live to bring itto a successful conclusion.

The Chairman: I am sure you will all agree with Mr. Stone that this is a most valuable paper. I know from my own knowledge that it has taken the Astronomer-Royal a very long time to prepare, and I hope he will be rewarded in producing a result which will receive the confidence of astronomers.

Mr. Neison: If I understand the paper rightly it is to show that Professor Adams, Delaunay, and Professor Cayley must have omitted something from their investigations on this subject, and that consequently their results are imperfect, but that Leverrier, and M. Pontécoulant, who took the opposite side to Prof. Adams, were right after all. Of course this is very important, and I hope Professor Adams will go over his work from the point of view indicated by Sir George Airy, and let us know what results he comes to.

The Astronomer-Royal: I am sure I join in the hope that that will be done, for it is a question worth a very serious investigation. Perhaps I may say I have attacked the problem in at least six different ways, and I found myself baffled in one after another, till at last I resorted to this, which was not known to me in the first instance. New difficulties seemed to come on time after time, till at last, five days ago, I perceived that these difficulties were overcome by one particular assumption of the last small terms. Till that very last term a difference uniformly existed, but that assumption set all right. All the preceding terms, it will be seen, were completed upon the absolute careful consideration of what might be expected under the circumstances, and upon that the assumptions were founded to determine their co-efficients. I declare I can hardly help laughing to myself when I think that so small a matter sets all right.

Mr. Christie read a paper by Mr. Ellery, of Melbourne, *On the great southern comet,* which was observed by him from the 9th to the 17th of February. After its first appearance it became fainter, though its tail increased to 45° in length; about the 12th February it almost disappeared. On the 17th the nucleus could only be observed with considerable difficulty with the great telescope in a perfectly dark field.

Mr. Christie also read a paper on the same subject from Mr. Russell, of Sydney. The comet was first seen by him on Sunday, the 1st of February, when its tail was 3° wide. Next day it was fainter. He observed it up to the 14th when he lost it. On the 13th the comet could with difficulty be seen by the naked eye, and on the 14th it was fading rapidly.

Mr. Christie said observers seem to have been very unfortunate in Australia. I think Mr. Gill saw the comet up to the 23rd of February, or thereabouts, but we have no official communication from him, only a letter to the Astronomer-Royal. I am sorry the President is absent, but I have received some very interesting letters from him. Mr. Hind has worked out the orbit of this comet and finds a remarkable resemblance between its elements and those of the comet of 1843. He treats the question from several points of view, taking only approximate places. Professor Weiss, of Vienna, has also discussed the matter, and on the assumption that the perihelion passage would be on January 27.6 of this year, he finds a close agreement with Mr. Gill's rough places, which are only given in minutes of time and minutes of arc of polar distance. I have also a letter from Mr. Hind, in which he says that Professor Winnecke appears to be of opinion that the comets of 1843 and of 1880 are the same. He also suggests a search for the observations of the 1695 comet, in Paris, upon which Burckhardt founded an orbit different from that of the comet of 1843. He thinks the elements of the comet of 1843 are incompatible with the observations of 1695, and the observations of Delisle were accompanied by too large errors to allow a close comparison. It appears then that the comet of 1880 is probably identical with the comet of 1843, and Professor Weiss thinks it may be also the comet of 1106, which is carrying matters rather far back, though the scanty account we have of that comet is similar to this one. Mr. Hind speaks of the 1695 comet, but there was another in 1689, also supposed to be identical with the comet of 1843. The observations of the 1843 comet seem to have been quite insufficient to determine its period with any degree of certainty. The most accurate determination appears to be that of Hubbard, who gave it a period of 533 years, but periods of all sorts have been found, from 7 to 500 years and upwards. In fact, a parabolic orbit would satisfy the observations very fairly. The comet of 1689 appears to have elements resembling those of the comet of 1843. There was also a comet in 1582 not very different from these, and another in 1577, but I should not like to pronounce any opinion myself on the point of identity. There is one point about this comet: that though on account of its inclination it is not likely to be very much disturbed by any of the planets of our system, yet in going so near the sun it might get mixed up with the corona, if not with something still more intimately connected with the sun, and if so, there is no saying what changes it may have undergone.

Mr. Neison asked whether he was correct in supposing that the observations of the 1843 comet were not good?

Mr. Christie said that the comet passed so close to the sun that the observations could not fix the period with any very great accuracy. The comet was not observed till after perihelion, when it was seen at noon-time in 1843 very close to the sun as a pale object something like the moon. The observations of the comet were rather rough and did not extend over a large arc of the orbit.

Mr. Chambers said his recollection of the observations of 1843 was that they were very defective. A period as high as 3,000 years was got out by some one.

Mr. Christie said: As far as I am aware there is no comet that would answer to those of 1880 and 1843 in the interval between 1843 and 1695 or 1689. The very close approach to the sun, and the very small perihelion distance mark this comet out from most of those which have preceded it.

Mr. Marth observed that the comet of 1668 had not been mentioned. Henderson had found that the rough observations made in 1668 at Goa, in India, could be represented within a degree by the elements of the comet of 1843. Supposing the comet of 1668 to be the same as that of 1843, it would not be very wonderful that it should reappear now after 37 instead of 175 years. The velocity of a body moving in the solar system depended simply on its distance from the sun and on the major axis of its orbit. If the velocity were reduced by a resisting medium there would be a reduction of the major axis, and there was nothing unreasonable in the supposition that however weak the corona might be, its resistance would have a very great effect upon the motion of a comet rushing through it, so that we ought not at all be surprised if it should turn out that the comet of 1880 is the same as that of 1843 and that of 1668, and if its time of revolution were so much affected that it might return already in, say, 17 years.

Mr. Common said he had searched for the comet with his 3-ft. reflector, aided by the ephemeris prepared by Mr. Hind, but without effect.

Mr. Jenkins: I cannot understand how for the comet of 1843 they got periods varying from 37 years to 3,000 and a parabola, while for this comet of 1880 they get a definite period, like 37 years.

Mr. Christie: This period of 37 years is inferred only from the perihelion passages in 1843 and 1880. The parabolic elements of the comets of 1843 and 1880 agree so closely that their identity appears very probable.

Mr. Marth: If the comet returned after a certain number of years and a half, it would come to perihelion somewhere in July or August under circumstances which would make it invisible from the earth, so that there might have been one or several returns between 1668 and 1843, without having been perceived. But it was possible there might have been none, and yet that at the next revolution it might have come back after only 37 years, because the slightest resistance so close to the sun would have such an enormous effect upon the major axis as to account for the difference.

Mr. Ranyard: With respect to the resistance offered by the corona, I should be very glad if more accurate information could be obtained with regard to the orbits of these comets, for ultimately it might throw a great deal of light on the constitution of the corona. We know that when a meteoric mass enters the earth's atmosphere it becomes white hot from the resistance experienced at a height of 70 miles above the earth's surface, and it seems impossible to suppose that flights of stones could pass with enormous velocities through the corona, at a height of much less than a solar radius above the photosphere, as the comet of 1695 was supposed to do, without experiencing very great resistance. It is very evident that in the region of the chromosphere there is very great gaseous resistance. This is evident from the shapes of the prominences, and very remarkable evidence of the existence of a resisting medium was afforded by the great prominence which was observed by Professor Young to be shot upwards to a height of more than six minutes above the photosphere. Mr. Proctor, in a paper in the *Monthly Notices*, compared the heights of the prominence at successive intervals of time as it was shot up with the height which a projectile would attain if shot up under the influence of the sun's gravity from the level of the photosphere, with an initial velocity of 250 miles a second, and he showed that the rapid rate at which the prominence attained its altitude proved that the prominence was being shot upwards within a resisting medium, and the curving structures of the corona also seem to give evidence of a resisting medium at much greater altitude, than those reached by the prominence structures. We have nothing at present to guide us with regard to the law of the decrease of density in the solar atmosphere.

Mr. Marth observed that the perihelion distance of the comet of 1843 from the sun's surface was about a fifth of the sun's radius. If Prof. Young observed a protuberance six minutes high, it would amount to that or more than that. Against the probability of the solar corona having such an effect it might be said that it had not been shown by the comet of 1680, which approached the surface to one-third of the radius, and which was observed before and after perihelion. Unfortunately there was only one useful observation made before perihelion. This single observation, made by Kirch, was reconcilable with the observations after perihelion, so that the effect of the approach to the sun to a third of the radius had not been sensible in this observation of 200 years ago. The elements of the comet of 1880 deduced from the rough places, which had been furnished by the southern observer, and those of the comet of 1843, were so close as regards the node and inclination, and the distance and direction of the perihelion, that it was really difficult to surmise that they should be different comets. A merely resisting medium would have no direct effect upon the node and inclination, but would merely affect the velocity of the comet, and consequently the major axis and eccentricity of its orbit.

Mr. Common then read a paper *On the nebula in the Pleiades*, which he had observed with his 3-ft. reflector. He made a drawing of it on the black board, showing it to consist of several distinct portions, materially different from the drawing of the nebula as given by Mr. Maxwell Hall in his paper in the *Monthly Notices*.

Mr. Marth said he would like to know if anybody had observed the passage of the middle of the red spot across the central meridian of the planet in 1878, as it would be useful to him to know it in the preparation of his ephemeris for Jupiter for the present year.

Mr. Christie said there must necessarily be great uncertainty about the rotation period of Jupiter, because there was a possibility of the red spot drifting, and the drift might be variable.

The following papers were also taken as read :—

E. J. Stone : *On mean refractions.*

Lord Lindsay : *On the relative star magnitude of Mars in February and March*, 1880.

D. W. Barker : *Observations of meteors, July*, 1879, *to March*, 1880.

T. D. Brewin : *Rotation period of Jupiter.*

REVIEWS.

The Solar Spectrum in 1877 *and* 1878, *with some practical idea of its probable temperature of origination.* By Piazzi Smyth, F.R.S.E., Astronomer-Royal for Scotland. From the transactions of the Royal Society of Edinburgh. Vol. XXIX. Edinburgh : Neil and Company. 1879.

This extensive tabular conspectus of the solar spectrum at an average altitude of 70°, from the ultra red to the ultra violet, visible to the eye through glass lenses and prisms, must, we think, be highly appreciated by spectroscopists. It is a most elaborate affair, and we should have been glad of space to do justice to it. It was made under very favourable circumstances, and increases Angstrom's total number ot lines from 1,400 to 2,000 nearly. An appendix gives an interesting account of the determination of the solar temperature, by Professor S. P. Langley, of the Alleghany Observatory, U.S., which he considers must be at least above, and probably very much above, 300,000° Fahr., or, taking the light radiations as more trustworthy, the solar superficial temperature at every point of its area must amount, on a mean, to more nearly 15,000,000° Fahr.! It has been previously estimated at very various amounts, from the 9,000,000° Fahr. of Sir John Herschel, to the 2,800° of M. Violle, of Grenoble, this last being lower than that for melting platinum.

Thirty-fourth Annual Report of the Director of the Astronomical Observatory of Harvard College. Presented to the Visiting Committee Dec. 5, 1879. By Edward C. Pickering. Cambridge University Press: John Wilson and Son. 1880.

Annals of the Astronomical Observatory of Harvard College. Vol. XI., Part II. Photometric observations made principally with the equatorial telescope of 15-in. aperture, during the years 1877—79. By Edward C. Pickering, Director, aided by Arthur Searle and Winslow Upton, Assistants in the Observatory. Printed from the Sturgis Fund. University Press: John Wilson and Son, Cambridge. 1879.

We rejoice that the subscription alluded to in the last annual report of this very active observatory has been completed by the generosity of the ladies and gentlemen whose names are appended, effectually remedying a state of things in which only a single instrument could be kept in moderate activity. Now both the meridian circle and the large equatorial are well employed, and arrears of work done either published or in course of publication. We notice, amongst many other important observations, that the zone of 8,000 stars, between 50° and 55° N., which fell to the share of the Harvard Observatory was completed on Jan. 26, 1879. It has occupied Professor Rogers for over eight years. The determination has been commenced of the light of all the stars (about 4,000) visible to the naked eye in the latitude of the observatory, and for this a special photometer has been devised, by which the star to be measured is seen in the same field with the pole star, with the same aperture and magnifying power. We should like to have dwelt more on the many subjects of interest in this report, but we must pass on to notice the second part of the 11th volume of the Annals. Concerning the satellites of Mars, we find the mean brightness of Deimos was much greater at the opposition in 1879 than in 1877, and that its light on the following side of Mars exceeds by nearly half a magnitude that on the preceding side. Its concluded diameter is equivalent to about six miles, the diameter of Phobos to about seven miles. Even as seen from Mars itself, Professor Pickering says that their phases would scarcely be recognised by the unassisted eye. We are struck by the immensity of ingenuity, care, and labour bestowed on these light measurements. Jupiter's satellites, compared with their primary, have the following relative magnitudes: I., 8·13. II., 8·27. III., 7·6. IV., 8·89. These observations do not confirm the common idea of the variability of their light. The relative magnitude of the satellites of Saturn and their primary, is found as follows: 1. Mimas, 11·91, equivalent diameter in miles 292 ± 9. 2. Enceladus, 11·40 and 370 ± 10 miles. 3. Tethys, 10·46 and 570 ± 18 miles. 4. Dione, 10·57 and 542 ± 17 miles. 5. Rhea, 9·88 and 745 ± 27 miles. 6. Titan, 8·50 and 1406 ± 52 miles. 7. Hyperion, 12·81 and 193 ± 5 miles. 8. Japetus (mean) 10·80 and 486 ± 4 miles, (max.) 10·44 and 574 miles, (min.) 11·85 and 307 miles. And of the satellites of Uranus, Titania 8·79 and 586 ±

16 miles. Oberon, 8·95 and 544 ± 15 miles. Satellite of Neptune, 5·93 and 2260 ± 60 miles. "The large equivalent diameter of the satellite of Neptune is noteworthy." The magnitudes of the planets at mean opposition may be expressed by the following numbers: Mars, —2·25, Jupiter, —2·52, Saturn, ·93, Uranus, 5·46, Neptune, 7·89. And reduced to a scale of stellar magnitudes the magnitudes of the various satellites in order of brightness are as follows: Satellite III. of Jupiter, 5·24; I., 5·61; II., 5·75; IV., 6·37; Titan, 9·43; Rhea, 10·81; Tethys, 11·39; Dione, 11·50; Japetus, at its mean brightness, 11·82: Phobos, 12·31; Enceladus, 12·33; Deimos, 12·61; Mimas, 12·84; Hyperion, 13·74; Satellite of Neptune, 13·82; Titania, 14·25; Oberon, 14·41.

The light of the six brightest stars is much greater than could be represented by 1·0. The following numbers have been assigned to them from the observations of Seidel and Wolff: *α* Aurigæ = 0·0. *β* Orionis = 0·0. *α* Canis Majoris = —1·5. *α* Canis Minoris = 0·5. *α* Boötis = 0·0. *α* Lyræ = 0·0. On pp. 289—90 is a catalogue of relative magnitudes of unequal double stars. The following are dimensions of a few of the asteroids with their standards of comparison and equivalent diameters, on the assumption that their albedo is equal to that of Mars: Pallas (λ Aquarii, 4·0 mag.), 8·82, 167 miles. Juno (*δ* Capricorni, 3·0) 9·87, 94 ± 4 miles. Vesta (Mars, —1·8) 6·51, 319 ± 10 miles. Antiope (90) (Mars, —1·8) 12·10, 51 ± 3 miles. Brunhild (123) (*β* Arietis, 3·0) 13·25, 20 ± 3 miles. Eva (164) (*β* Aurigæ, 2·0) 13·90, 14 ± 1 mile. Menippe (188) (*γ* Libræ 4·4) 14·97, 12 ± 1 miles.

Solar Parallax from the Velocity of Light. By D. P. Todd, M.A., assistant *Nautical Almanac.* (From the *American Journal of Science*, Vol. XIX., January, 1880.)

Mr. Todd is of opinion that the velocity of light will at no distant time define the solar parallax within very much smaller limits than astronomers have yet known. From a combination of all the determinations beginning with Fizeau in 1862, and ending with Michelson in 1879, he finds the most probable velocity of light is 186,360 miles per second. Combining this value with the theory and observations of the satellites of Jupiter, and the value of the equatorial radius of the earth by Listing (Gottingen, 1878) = 3962·790 miles, there results the parallax of the sun 8″·802. Next, combining the above velocity with Struve's constant of aberration, 20″ 4451, and other elements of the terrestrial orbit, the parallax is found 8″·811. Finally, the consideration of the probable variation of the elements of this computation and their effects leads to the conclusion that all the experimental determinations of the velocity of light hitherto made, give, when combined with astronomical constants, the mean equatorial horizontal parallax of the sun, 8″·808 ± 0″·006. The corresponding mean radius of the terrestrial orbit is 92,800,000 miles.

Note sur la Tache rouge observée sur la planète Jupiter pendant les oppositions de 1878 *et de* 1879. Par L. Niesten, Astronome à l'Observatoire royal de Bruxelles. Bruxelles: F. Hayez. 1879. pp. 20.

In this memoir the author shows that at different intervals during two centuries a spot has been visible on Jupiter which appears to be identical with the one which has lately attracted so much attention. His own observations are best represented by a rotation of 9h. 55½m. He refers to observations by Cassini in 1665, and by Maraldi in 1708, and recent ones in America and this country. The period between two consecutive returns of the spot seems to be between five and six years; that is, in a revolution of Jupiter (11·86 years) it would attain its maximum of intensity twice; one maximum when the planet reaches a heliocentric longitude of 324°, or is 50° from its perihelion, as Maraldi indicated; the other at a longitude 157° when it is near its aphelion. Various questions are suggested concerning this enigmatical marking—"a spot at once fixed and transient," as Maraldi called it. Does it indicate a permanent feature on Jupiter, concealed at certain times by an atmosphere more or less dense, in this respect reminding us of the variations of the spots on Mars? This and other inquiries must be settled by future observations. Drawings accompany this note, by Cassini, 1665; Secchi, 1857; Lassell, 1858; Gledhill, 1870—71; and one (coloured) by Prof. Niesten himself, 1879, made with an equatorial of 6-in. aperture, and power 270, which seems to us very well done, and in its larger features reminds us of what we saw with a much smaller instrument.

On the coincidence of the bright lines of the oxygen spectrum with bright lines in the solar spectrum. By Henry Draper, M.D.

On photographing the spectra of the stars and planets. By the same. (From the *American Journal of Science and Arts,* Vol. XVIII., Oct. and Dec., 1879,)

To the first of these papers is appended the discussion which followed its reading before the Royal Astronomical Society, of which a report was given in the *Astronomical Register.* Dr. Draper says, "In closing, it may be well to give some idea of the amount of labour and time this research has already consumed, and this cannot be better done than by a statement of the production of electrical action that has been necessary. Each photograph demands an exposure of 15 minutes, and, with preparation and development, at least half-an-hour is needed. The making of a photograph, exclusive of intermediate trials, requires, therefore, about 30,000 10-inch sparks, that is, 30,000 revolutions of the bobbin of the Gramme machine. In the last three years the Gramme has made twenty millions of revolutions. The petroleum engine only consumes a couple of drops of oil at each stroke, and yet it has used up about 150 gallons. Each drop of oil produces two or three 10-inch sparks. It must also be borne in mind that comparison spectra can only be made when the sun is shining, and clouds, therefore, are a fertile source of loss of time."

In the second paper the author observes that a preliminary examination of the results he has obtained at once shows that these stellar spectra are divisible into two groups: first, those closely resembling the solar spectrum, and, second, those in which there are relatively but few lines, and those of great breadth and intensity. The photographs of the spectra of Arcturus and Capella are so similar to the solar spectrum, that as yet no material differences have been detected. On the other hand the spectra of Vega and α Aquilæ are totally different. In the atmosphere of Vega hydrogen exists to a large extent. In the photographs of its spectrum there are eleven lines, only two of which are certainly accounted for, two more may be calcium, the remaining seven, though strongly resembling hydrogen lines are as yet not identified. The spectra of the heavenly bodies offer to us conditions of temperature and pressure that cannot be attained by any means known at present on the earth. But in the stars, nebulæ, and comets, there is a multitude of experiments all ready performed for us, with a variety of conditions of just the kind we need. To observe and interpret these results is the object of Dr. Draper's laborious and valuable investigations, and from them and those of Dr. Huggins and others we may hope for future discoveries of importance, not only in relation to celestial, but also terrestrial chemistry and physics.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE DOUBLE STAR Σ 2318.

In my recently-published double star observations (Mems., Royal Astronomical Society, Vol. XLIV., p. 140) I have given all the measures of this pair, and added some remarks, as follows:

Σ	P = 257°·1	D = 12″·85	1829·7
Ma	256·8	20·03	1843·7
De	255·7	20·66	1865·1
Hd	255·3	20·85	1866·7
β	254·9	20·81	1878·4

"There seems to be a slow retrograde movement in angle, but it is evident the distance could not have been as given by Σ, if the later measures are correct. The close agreement in all the later distances makes it almost certain that there is an error of some kind in Σ. I have already suggested (*Astronomische Nachrichten,* 2210) that a mistake in a single figure in transcribing or printing the distance of Σ may have occurred, and if we suppose the figures to be 19″·85, instead of 12″·85, the whole series of observations would substantially harmonize. I hope the original records will be examined to see if this is the case. I have carefully looked for any other pair in this vicinity, agreeing with Σ's description, and find none."

Since this was printed I have learned from O Σ that the original observations by Struve have been examined, and the suspected error found to really exist. The mean distance should be 20″·51, instead of 12″·85. This disposes of the mysterious change in this pair.

Chicago: March 17, 1880. S. W. BURNHAM.

THE GREAT SOUTHERN COMET OF 1880.

Sir,—Other readers besides myself may have noticed the curious discrepancies in the descriptions of this comet in the *Monthly Notices*. D. Gill's account of the lower part of the tail on Feb. 6 and 7, and C. Todd's on the latter date, while agreeing together, being nevertheless inconsistent with all the descriptions of the nucleus. On both these evenings, Gill, while not having observed the nucleus, draws the tail considerably past where the nucleus must have been; it is true he draws it partly with dotted lines, indicating that the observation was uncertain, but on the 7th several degrees of the portion below the nucleus are not so drawn. Todd also on the 7th says distinctly that the tail then passed over Fomalhaut. Can the comet have had two tails in opposite directions?

Yours truly,
Sunderland: April 12, 1880. T. W. BACKHOUSE.

Errata, on page 92.—Line 5, for A.D. 1880, read 1830. Column D. Fifth number from bottom, for 233, read 253; second number from bottom, for 360 read 350.

THE TOTAL ECLIPSE OF JUNE 22nd.

Sir,—Be good enough to allow me to direct the attention of foreign and colonial astronomers to the total lunar eclipse of June next.

In this eclipse the moon may disappear completely from view. Observations of such eclipses have been very rare. Within the past 700 years there are records of only six, and of all of them it was observed that during the eclipse "nothing of the moon was seen." These eclipses all occurred in April, May, or June.

I am, Sir, your obedient servant,
Dulwich: April 12. B. G. JENKINS.

MARS AND α ORIONIS AND α TAURI.

Sir,—I do not know whether the following rough eye-estimations of the relative magnitudes of Mars, α Orionis, and α Tauri, made during the end of February, March, and the beginning of April, will have any value after Lord Lindsay's photometric observations made at Dun Echt.

I was as careful as possible in estimating the different magnitudes, but this was a very delicate matter, especially when the stars approached each other in brightness. I have not made any allowances for differences of altitude, &c., but give simply the appearances at the dates named.

Atmospheric circumstances were favourable unless otherwise stated.

Feb.

d.	h.	m.	
21	10	30	Mars quite distinctly brighter than α Orionis.
28	10	0	Mars slightly brighter than α Orionis.
Mar.			
6	8	0	Mars still slightly brighter than α Orionis, but the difference was very small.
9	7	0	
10	8	30	I came to the conclusion that Mars and α Orionis were sensibly equal.
13	6	30	Strong twilight and a slight mist; α Orionis seemed remarkably faint, and I thought Mars was perhaps, if anything, brighter than it.
15	8	0	α Orionis slightly brighter than Mars, which was about as much brighter than α Tauri; good observation.
25	8	0	Mars seemed rather brighter than α Tauri.
26	7	0	
28	9	30	α Tauri still rather fainter than Mars, but the latter had the better position.
April			
1	7	50	Mars appeared now less bright than α Tauri, good observation.
2	7	45	Mars and α Tauri very equal, perhaps something in favour of the latter, though it was in a brighter sky.
3	8	10	Mars was plainly less bright than α Tauri.
6	8	0	

I may add that on several occasions the apparent variability of α Orionis struck me; it was very slight, but I think it certainly exists. My brother, Mr. John McCance, observing at Davos-Platz, in Switzerland, thought Mars and α Orionis about equal on March 10, 8h., and again on 11d., 7h. 30m., he remarks on the similarity in brightness of α Tauri and α Orionis.

I remain, Sir, yours faithfully,

JAMES L. McCANCE.

Rathfern, Rayner's Road,
Putney Hill: 1880, April 16.

THE "CANALS" OF MARS.

Sir,—Permit me to commence this letter with an apology to my excellent friend Mr. Green, for dissenting from some of the conclusions to which he appears to have come respecting the so-called "canals" of Mars. The weather prevalent here during the planet's apparition was often exceedingly favourable for close scrutiny of his surface under magnifying powers up to 514 diameters, on the 12-in. reflector. Both with this instrument and my 8-in. reflector, as well as with a fine 6-in. by Grubb, I repeatedly and distinctly saw narrow dusky streaks traversing certain of the Martian continents, very strongly resembling in their aspect, and two of them identical with, Dawes Strait and Huggins Inlet, as drawn by Messrs. Dawes (1864) and Knobel (1873). Huggins Inlet was seen several times under exquisite definition as a ribbon-like streak of a greenish blue tint, with sharply-defined undulating or serrated edges. Besides the two streaks above named, many others were seen, chiefly in the neighbourhood of Christie Bay and the Terby Sea. All those seen were comprised within the area included between the meridians of 0° and 280°, and the parallels of 20° S. and 45° N. Dr. Schiaparelli has accepted the drawings of these objects made here, as representing the objects named by him "canals," and most valuable support has been given to my seeings by the drawings obtained by Mr. J. L. E. Dreyer with the "South" equatorial at Dunsink. Mr. Dreyer's drawings and my own, with the accompanying notes and provisional chart, representing the details presumed to be permanent, are now in course of publication by the Royal Dublin Society, and will shortly be in the hands of those interested in the subject. With all deference to Mr. Green's high authority in artistic matters, I cannot but think that he has been mistaken in supposing that the "canals" of Dawes and Schiaparelli are merely the boundaries of tints or shadings. One or two of them seemed to me to possess as decided a character as Nasmyth Inlet itself, and to present the same characteristic tint and serration of edge as that displayed, *e.g.*, by the northern edge of Maraldi Sea or the Kaiser Sea.

I am, yours truly,
C. E. BURTON.

THE TRAPEZIUM IN ORION.

Observing with a 5½-in. refractor several years ago, I never saw more than the fifth star distinctly. Later, with an 18-in. reflector I always saw both on favourable occasions, but unless the night was fine, with so much difficulty that I can quite understand why they were overlooked for so long. I had always looked upon them quite as atmospheric tests for anything that once showed them.

On the morning of September 22, about 4h., after I had been observing Mars, the sun then brightening up the sky, I turned the 3-ft. telescope on Orion, for a glimpse of this object with the new glass, then just mounted. The morning was most beautifully clear, and I was quite astonished at what I saw. The trapezium was so no longer, for there were plainly seven stars to be seen without any difficulty, in fact, so apparent that they could not be overlooked. The seventh star was beyond the sixth, towards the middle of the three bright stars, to the S. *f.*, about twice as far as the sixth from *a*. This was with a power of something like 120. (I put on higher powers and closely scanned the middle of the trapezium, having in mind a request from Dr. Huggins, but without seeing anything.) This seventh star is no doubt 636 of Bond's. I then thought that I should be able to see all I had seen again, but looking again, later on in the autumn, I quite failed to see this seventh star, although the fifth and sixth and a number of smaller ones that I had noted down roughly were visible. One fine night it was glimpsed, but not steadily seen. Bond gives two stars not very far from the fifth, these I have never seen, though they may of course be fainter than 636, and require careful looking for, a useless proceeding on any but a very fine night, of which there has been quite a scarcity since last September. After this my opinion is, as may be supposed, that the clearness of the sky has all to do with the ease with which the fifth and sixth stars can be seen. Of the other fainter stars, excepting a reddish-coloured one near *n. p.* edge of the bottom of the "fishes mouth," I have never noticed any variability. This reddish star I think Bond's 651, and called variable by him, has certainly been seen brighter than at other times, using the faint stars south of the trapezium as a guide.

Ealing: April 15. A. A. COMMON.

LESSONS IN SELENOGRAPHY.

By the Rev. W. J. B. Richards, F.R.A.S., and W. R. Birt, F.R.A.S.

Chapter II.—Section II.

Having directed attention to the Mare Crisium as typical at least of the enclosed maria, of which Neison specifies two as completely enclosed, we pass on to the consideration of those formations respectively styled *Palus* (marsh), *Lacus* (lake), and *Sinus* (bay). These have generally been considered to reflect less light than the *Maria.* As examples of Lakes we select the *Lacus Somniorum,* and the *Lacus Mortis:* the first is situated north of the ring plain *Posidonius,* 123 on our map, its eastern portion merging into the north-western portion of the *Mare Serenitatis.* On the *Lacus Somniorum* are a few interesting craters, *Daniell* and *Grove* situated a little west of the meridian of 30° west longitude, which is the second meridian from the west limb or margin of our map. This meridian passes through a tolerably sized crater, *Mason,* having a much larger one on the east, *Plana,* both of which are situated on the south-east border of the *Lacus Mortis.* This formation is described by Neison as "a small gray plain between 40° and 50° north latitude, it is marked D on our map, and has, immediately to the west, the two large craters *Atlas* 10 and *Hercules* as 11, the meridian 40° west passing between them. Neison describes *Atlas* a great ring plain, nearly 55 miles in diameter, having a complex broad wall broken by cavities, valleys, and passes, rising at intervals into lofty peaks, and reaching on the west an altitude of 8,895 feet, on the north 10,944 feet, and on the east 10,443 feet above the interior. The floor is of different tints, indicative of different reflective powers, and, it may be, different substances. The same author describes *Hercules* as a magnificent ring plain, with a diameter of over 46 miles, surrounded by a wall rising to 10,889 feet on the west and 10,673 on the east above the interior. South of *Hercules* is a table-land covered with low mountain ridges. These magnificent ring plains form a fine example of a peculiar class of lunar formations known as twin-craters, which vary in size from the magnificent objects just alluded to, to some of the smaller craters occurring in pairs on the surface of the moon.

MOUNTAINS.

Isolated mountains. One of the finest examples of this class of lunar mountains is *Pico,* on the south of *Plato,* No. 78 on the map; it is situated on the south part of the ring of a slight depression, presenting every appearance of having formerly been a walled plain on the south of and adjoining *Plato,* the wall of which has disappeared, but some fragments have been left, of which Pico is the principal. Its height is about 8,000 feet. The depressed surface named by Schröter *Newton,* is shown on our map just above or south of *Plato.* There are several mountains hereabout more or less detached, those called the *Teneriffe Mountains* being nearest to *Plato,* their locality is marked *k* on our map. Neison describes them as "a fine mountain group, extending for 60 miles," many of them attain a brightness of 7°. South of *Pico* is an interesting isolated mountain, with three peaks not shown on our map, but marked B on Neison's map VI. This mountain, as well as *Pico,* attains a brightness of 7°. According to Neison its height is nearly 4,000 feet, but Schröter rates it at 6,000. To the east of the *Teneriffe Mountains* is a fine distinct line of mountains, the highest peak rising to the height of 6,254 feet about midway between *Plato* and the *Sinus Iridum,* S on our map, the western extremity at sunset throws a fine needle-like shadow.

Mountain Ranges. Several of the ranges of lunar mountains are of an extensive character, those specified on the map given in our March number are thus designated:

a The Pyrenees
b The Altai Mountains
c Mount Taurus
d The Alps
e The Caucasus
f The Apennines
g The Carpathians
h The Riphæans
i The Percy Mountains
k The Teneriffe Mnts.
l The Harbingers
m The Hercynians

The following ranges form an almost continuous chain, viz., The *Alps*, *d*, the Caucasus *e*, the *Apennines f*, and the *Carpathians g*. Starting from the west of *Plato*, we have the Alps, which attain an altitude of at least 12,000 feet in the neighbourhood of the mountain mass named *Mont Blanc* by Schröter. This collection of mountains (the *Alps*) extends from *Plato* to a point north of *Cassini* 62, and is broken by a remarkably deep valley 83 miles in length, which attains in some places a width of six miles. Neison states that this great valley unites the *Mare Imbrium* Q with the *Mare Frigoris* R. These Alpine masses conduct us to the north-east boundary of the *Mare Serenitatis*, C on our map, which is skirted by the *Caucasus e*, and communicates with the *Palus Nebularum* and *Palus Putredinis* by a steppe, so that the level of the *Mare Serenitatis* is higher than the surface to the east; this steppe can be well observed shortly after sunrise as a dark mark, being in shadow—completing the boundary of the *mare*, under the evening illumination, it is seen as a bright mark, the sun shining full upon it. This steppe is not shown by Neison, but mentioned in his text. Between the *Palus Nebularum* and *Palus Putredinis* are two ringed plains *Autolycus* 60 and *Aristillus* 61. The ring of the smallest to the south is described by Neison as rising on the east to 9,017 and on the west to 8,358 feet above the interior, while at an intermediate point it attains only 4,777 feet above the *Palus Putredinis*. The west wall of *Aristillus* rises 8,837 feet above the floor and 5,065 feet above the *Palus* [*Nebularum*]. The eastern wall is much steeper, attaining 11,152 feet above the interior; these ring plains with the furrowed sides of Aristillus, are exceedingly interesting, they present, shortly after the first quarter, one of the finest lunar landscapes. The fine range of the *Caucasus*, the *Mare Serenitatis*, with its steppe, the splendid range of the *Apennines*, stretching towards the south-east, together constitute a view not easily forgotten by the young enthusiastic selenographer. The *Carpathians* carry this chain as far as 30° of E. longitude, which with the continuation of the high land east of *Plato* to the Sinus Iridum, S on the map, also in 30° E. longitude, completely enclose on the north, west, and south the *Mare Imbrium* Q.

Before leaving the subject of mountains we desire to call attention to the existence of a track of high land which presents many features of an ancient formation. We extract the following from the report of the Lunar Committee of the British Association for the Advancement of Science (Report 1869, p. 80), "A very ancient formation has been traced 5° and 10° S. latitude, and 5° and 10° W. longitude, the earliest state of which is considered to have been very similar to many of the more recent districts, such as those in which perfect craters and mountainous regions intermingle. The first change which appears to have taken place in this formation is that of the production of a grey plain, traces of which still exist. The material of this plain appears to have invaded certain craters, breaking down the walls of those immediately facing the plain and partially filling others. The next change appears to have been of an elevating character, the evidence consisting of a line of low mountains which has in a great measure obliterated the characteristics of a grey plain, and introduced those more in accordance with an ancient district, which are strikingly in contrast with the features of the recent craters to the east. The only instance of volcanic outburst on this ancient district consists of a chain of craterlets of quite an insignificant character."

(*To be continued.*)

ASTRONOMICAL OCCURRENCES FOR MAY, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Libræ.
Sat	1	1 52 3 7	☾ Moon's Last Quarter Conjunction of Saturn and Venus 0° 47′ S. Conjunction of Uranus and Sun			12 3·1
Sun	2		Sidereal Time at Mean Noon 2h. 39m. 9·68s.			11 59·2
Mon	3		Sun's Meridian Passage 3m. 17·92s. before Mean Noon			11 55·3
Tues	4					11 51·4
Wed	5	14	Conjunction of Moon and Jupiter 6° 47′ S.	4th Tr. E.	16 9	11 47·4
Thur	6	2 19 21	Conjunction of Saturn and Mercury 0° 41′ S. Conjunction of Moon and Saturn 7° 41′ S. Conjunction of Moon and Mercury 8° 18′ S.			11 43·5
Fri	7	8	Conjunction of Moon and Venus 6° 32′ S.	1st Ec. R.	15 58 39	11 39·5
Sat	8	18 16	● New Moon	1st Sh. E.	15 31	11 35·6
Sun	9					11 31·7
Mon	10					11 27·7
Tues	11					11 23·8
Wed	12	9 1	Near approach of 6 Geminorum (6)			11 19·9
Thur	13	21	Conjunction of Moon and Mars 2° 39′ N.	4th Ec. R.	15 17 22	11 15·9
Fri	14		Saturn's Ring: Major axis=36″·73 Minor axis= 8″·71			11 12·0
Sat	15		Illuminated portion of disc of Venus=0·961 Illuminated portion of disc of Mars=0·934	1st Sh. I. 3rd Tr. E.	15 9 16 6	11 8·1

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Sun	16	22 23 10 47 11 6	☽ Moon's First Quarter Occultation of *h* Leonis (6) Reappearance of ditto	1st Oc. R.	15 35	α Libræ. — 11 4·1
Mon	17		Sidereal Time at Mean Noon 3h. 42m. 14·57s.			11 0·2
Tues	18	 7 57 9 11	Sun's Meridian Passage 3m. 44·81s. before Mean Noon Occultation of p^2 Leonis (6) Reappearance of ditto	2nd Sh. E.	14 39	10 56·3
Wed	19	16	Conjunction of Venus and Mercury 0° 25′ S.			10 52·3
Thur	20					10 48·4
Fri	21					10 44·5
Sat	22					10 40·5
Sun	23	18 38 12 26 13 37 14 53 15 31	○ Full Moon Occultation of B.A.C. 5253 (6) Reappearance of ditto Occultation of B.A.C. 5286 (6½) Reappearance of ditto	1st Ec. D.	14 15 9	10 36·6
Mon	24			1st Tr. E.	14 54	10 32·7
Tues	25	9	Uranus at quadrature with the Sun	2nd Sh. I.	14 29	10 28·8
Wed	26					10 24·8
Thur	27			2nd Oc. R.	14 34	10 20·9
Fri	28					10 16·9
Sat	29					10 13·0
Sun	30	10 53	☾ Moon's last quarter			10 9·1
Mon	31			1st Tr. I. 1st Sh. E.	14 38 15 43	10 5·1
JUNE Tues	1	13 18 14 1	Occultation of B.A.C. 782 (6½) Reappearance of ditto	1st Oc. R.	14 2	10 1·2

THE PLANETS FOR MAY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	1 5 0	N. 3 42½	7″·1	22 22·2
	9th	1 46 45	N. 8 9	6″·3	22 32·4
	17th	2 37 43	N.13 29½	5″·6	22 51·7
	25th	3 39 38	N.19 0½	5″·2	23 22·0
Venus ...	1st	1 27 6	N. 7 30	10″·4	22 42·2
	9th	2 4 1	N.11 6½	10″·2	22 49·6
	17th	2 41 48	N.14 26½	10″·1	22 55·8
	25th	3 20 39	N.17 24½	10″·0	23 3·1
Mars ...	1st	6 50 56	N.24 32½	5″·8	4 11·1
	9th	7 11 18	N.24 0	5″·6	3 59·9
Uranus ...	4th	10 28 4	N.10 27½	4″·2	7 35·8
	16th	10 28 0	N.10 27	4″·2	6 48·6

Mercury rises half an hour before the sun, at the beginning of the month, the interval decreasing.

Venus rises about half an hour before the sun throughout the month.

Mars sets about half an hour after midnight on the 1st, the interval decreasing. On the last day he sets about half an hour before midnight.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN MAY, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

In the *Selenographical Journal*, No. 9, November 25, 1878, the Editor called attention to some craters situated to the west of *Parry* (Neison's map XIII., facing page 346 of his work on the moon) the largest of which is Parry B, in 13° longitude E., and 7° 40′ latitude S. It is drawn by Mädler with an inner diameter of 2½ miles and an outer diameter of five miles, and is described as being remarkably deep; north of it is the smaller crater, Parry c, likewise very deep, and not far off were a number of small craters. According to Schmidt these two remarkably deep craters have disappeared. Assuming that Neison's lettering is identical with Mädler's, we can compare some observations by Miss Mary Ashley, under date of 1878, December 3, 4, 5 and 6, with Neison's map, in which 11 craterlets are described. The observations were made with a 3¼-inch refractor, power 95. Miss Ashley says, "The objects were too far from the terminator to see their nature distinctly, but I compared them with Mr. Neison's map, and have traced the principal objects from it, and numbered the craters for reference, the letters are Mr. Neison's. I saw all the craters he has marked plainly on one or other of the nights, also another which he has not shown: B and *e* are the two largest [*e* is in a line with B and C, and is given by Miss Ashley as about the size of B; in examining this region for the missing craters the first step is to identify the three craters in a line C *e* B] and plainest, *e* is larger and whiter than B much whiter but not quite so much as half the size larger, B appeared plainly as a crater; it was sometimes less easily seen than *e*." Of Miss Ashley's numbered craterlets 1 and 2 are situated between C and *Gurike*, 3, 4, and 5 form a triangle north-west of C, the five are all north of C; 6, 7, and 8 were a good deal smaller, quite half the size and not so plain, 8 is the smallest; the three form a triangle between *e* and B; 9, which is north-west of B, appeared more directly in a line with B and A than Mr. Neison has it. The two north of B, *viz.*, 10 and *c*, were in a slightly different position than on Neison's map. 3, 4, 5, 6, 7, 8, 9 and 10 appeared on or near the edge of a dark patch; its tint was not very dark, and on one or two nights it was not nearly so plain as on the others. North-west of B there was what looked like a round depression, with sloping sides; it was seen once only and not very plainly.

On the 23rd of March, 1880, Mr. E. M. Richards writes from Grange Killann, Enniscorthy, Ireland, to the effect, "With 5⅛-in. equatorial, power 64 to 200 I examined [the region west of Parry] on the 19th, when close to the terminator, also on the 20th and 21st, with this result (using Neison's letters): B seems a hill, no signs of its being a crater. I saw a craterlet in or near the position of *c* on the 19th, it looked like a mound, but on the 20th internal cavity seemed plain. There are several objects in the vicinity of *c* and others not marked by Neison."

It would appear from the above observations that the two craters B and *e* are still in existence, Miss Ashley having seen B plainly as a crater, and Mr. Richards stating "that on the 20th of March, 1880, the internal cavity of *c* seemed plain." A further examination of this region is very desirable.

Erratum in last list, page 99, line 11, for *Allin* read *Allier*.

Water Lane, Stratford.
April 5, 1880.

THE COMET SCHABERLOON.

The following ephemeris of Comet Schaberloou has been received from the Vienna Academy:

Ephemeris for Berlin midnight.

1880	R. A.			Decl.		log. Δ	log. *r*	Brightness.
				°	′			
April 14	6	25	8	+78	30·2	0·3102	0·3146	0·97
18		18	27	75	32·2	0·3202	0·3111	0·95
22		15	30	72	44·2	0·3303	0·3078	0·92
26		14	32	70	6·1	0·3408	0·3047	0·89
30		14	43	67	37·8	0·3513	0·3019	0·86
May 4		15	38	65	19·2	0·3616	0·2992	0·83
8		17	2	63	9·2	0·3718	0·2968	0·80
12	6	18	45	+61	7·6	0·3816	0·2946	0·77

The brightness on April 10 has been taken as unity.

Dun Echt Observatory:
1880, April 21.

SATELLITES OF URANUS.

Major and minor semi-axes *a* and *b* of the apparent ellipses described by the satellites, and position-angles p_o of the major axes:

	Ariel.		Umbriel.		Titania.		Oberon.		
1880.	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	p_o
	″	″	″	″	″	″	″	″	°
May 4	14·76	2·77	20·56	3·85	33·73	6·32	45·11	8·45	13·36
14	14·63	2·75	20·38	3·83	33·43	6·28	44·71	8·39	13·36
27	14·50	2·71	20·20	3·77	33·13	6·18	44·30	8·27	13·37

The satellites, moving in the direction of decreasing position angles will be at their greatest northern and southern elongations N. and S. (in position angle 13°·4 and 193°·4), and at their inferior and superior conjunctions (in pos. 283° and 103°) about the following hours, G.M.T.:

Ariel.		Umbriel.		Titania.		Oberon.	
1880.	h.		h.		h.		h.
May 1	5·1 S.	May 2	20·3 N.	May 1	4·7 N.	May 1	6·5 inf.
2	11·4 N.	4	22·1 S.	3	8·9 inf.	4	15·3 S.
3	17·6 S.	6	23·8 N.	5	13·1 S.	8	0·1 sup.
4	23·9 N.	9	1·6 S.	7	17·4 sup.	11	8·9 N.
6	6·1 S.	11	3·3 N.	9	21·6 N.	14	17·8 inf.
7	12·4 N.	13	5·0 S.	12	1·9 inf.	18	2·6 S.
8	18·6 S.	15	6·7 N.	14	6·1 S.	21	11·4 sup.
10	0·9 N.	17	8·5 S.	16	10·3 sup.	24	20·2 N.
11	7·1 S.	19	10·2 N.	18	14·6 N.	28	5·0 inf.
12	13·4 N.	21	11·9 S.	20	18·8 inf.	Ariel.	
13	19·6 S.	23	13·7 N.	22	23·0 S.	May 20	1·8 N.
15	1·8 N.	25	15·4 S.	25	3·3 sup.	21	8·1 S.
16	7·1 S.					22	14·3 N.
17	13·3 N.					23	20·6 S.
18	19·6 S.					25	3·8 N.

A. M.

Books received.—Results of Astronomical Observations at the Melbourne Observatory. Melbourne: John Ferres. 1879.—Three Approximate Solutions of Kepler's Problem. By H. A. Howe.—Extra Meridian Determination of Time. By Ormond Stone.—Micrometrical Measurements of 1054 Double Stars, observed with the Cincinnati 11-in. refractor. Ormond Stone, Director. Cincinnati, 1879.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Mar., 1880.
Court, J.
Vines, D.

To April, 1880.
Burton, C. E.
Holland, P.
Pritchard, Rev. Prof.

To June, 1880.
Gray, J. P.
Noble, Captain W.
Ryves, E. W.
Walter, H. S.

To Dec., 1880.
Bazley, T. S.
Slater, J.

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The Astronomical Register.

No. 210. JUNE. 1880

ROYAL ASTRONOMICAL SOCIETY.

Session 1880—81.

The third Meeting of the Session was held at the Society's rooms, Burlington House, Piccadilly, on Friday, May the 14th.

J. R. Hind, Esq., *President*, in the Chair.

Secretaries—J. W. L. Glaisher, Esq., and W. H. M. Christie, Esq.

The minutes of the preceding meeting were read and confirmed.

Mr. Thos. Buckney, 61, Strand, W.C.
Mr. L. A. Eddie, Oatlands, Grahamstown, Cape of Good Hope.
Mr. Thos. Gullon, Northenden, Cheshire.
Signor N. Perini, 261, Hampstead Road, N.W.

were balloted for and duly elected Fellows of the Society.

Mr. Christie announced that 58 presents had been received by the Society since the last meeting. Amongst them was a MS. book, by the Rev. S. J. Johnson, showing the solar and lunar eclipses which would be visible in London till the end of the twentieth century.

The Astronomer-Royal was then called upon to read a paper *On the preparations to be made for observing the transit of Venus of* 1882, *December 6th.* He said: Before I enter on the subject of my paper I will ask your permission to make a remark in reference to the paper which I introduced at the last meeting. Just as I was starting for this meeting I received a note from Professor Adams, in which he pointed out to me an error in my paper; and I confess I am quite wrong, and Professor Adams is quite right in that matter. It happened it was the last sentence which

I really wrote, and, as usually happens in last sentences, there is a greater chance of error in them than anywhere else. But in regard to the subject of the paper, and the general treatment of it, I will say this—I treated it in a manner which I believe to be more correct and more powerful than any other mode. I was anxious to show its power in regard to the interesting matter of the acceleration of the moon's mean motion. If I discover any error in it I shall not be slow to avow it; but I have not yet found any, and do not expect any. The external factor may require examination.

With regard to the subject of his paper the Astronomer-Royal said: He believed that the scientific world would not be satisfied unless the opportunity was taken to secure all that could be obtained from the transit of 1882. It was an opportunity such as would not occur again until after the extinction of three generations. We shall approach this transit under conditions far more favourable than those of 1874. We have the personal experience of many observers still alive. A great step also has been gained by the extension of telegraphic communication; and, finally, by departing in some respects from the plan pursued before, we can adopt a course which seems to possess all the elements of success. The most important condition for observation is a sufficient elevation of the sun above the horizon. For this advantage it would be well, probably, to sacrifice something in the magnitude of the parallax factor; and he would fix the elevation at 20°, although 15° would be accepted as sufficient.

For ingress accelerated he would rely on Cape Colony, from the Cape Observatory to Durban. As regards absolute longitude he could not doubt that, with Mr. Gill, the longitude of the Cape Observatory would be accurately obtained, and with respect to correct longitudes of other stations, telegraphic communication exists on the whole coast. The sun's elevation would range from about 33° to 46°, at Mauritius about 10°, and in the south part of Madagascar 21°. The ingress accelerated will be visible in England and other parts of Europe, but the circumstances will not be favourable.

Ingress retarded he proposed to refer principally to the American coast; but as he knew that the elevation of the sun would be only 15° to 18°, which was too small, he would substitute the whole chain of the West Indian Islands, or stations on the neighbouring continent of Central America. At any of these stations it would be only necessary to determine local time, as the longitudes have been well determined. The sun's elevation ranges from about 23° to 43°; the factor of parallax is 0·75 to 0·85.

For egress accelerated all the stations suggested for ingress retarded would be adopted with advantage. The sun's elevation at Bermuda would be 13°, at Charleston 22°, in the chain of islands 25°, and at the continental stations 35°.

Egress retarded should be observed at Melbourne with an elevation of 10°, and at Sydney and the whole of the eastern coast of New South Wales with an elevation of 14°, besides New Caledonia with 22°; but he would be inclined to rely more completely on New Zealand, where, if the telegraphic communication should not be complete, they could determine the longitude by means of chronometers—a method in which he had learnt to repose great faith. The sun's elevation on the east coast of Australia would, perhaps, be 14°, at Hobart Town 14°, and in New Zealand from 30° to 35°. The factor of parallax for Australia would be 0·96, and for New Zealand 0·80.

At stations where the residence is to be for some considerable time, no instrument is so good as the old transit; but at other places where the observer has only to determine the state of the clock for a few days, the best instrument is a vertical circle Duplicates of the parts likely to be lost ought to be taken, and a good sidereal clock and solar chronometer, with a six-inch telescope, equatorially mounted, and protected against the sun's radiant heat. But for an accident the telescopes sent out for the 1874 transit would probably have been useless; for when observing with what he considered to be good dark glasses, one of them melted before his eye. The best plan was reflection from the external surface of glass, and whoever went out to observe this transit of Venus ought to have the telescopes ready in the summer of 1882, and well tried. A large proportion of these instruments could be supplied from the Royal Observatory. It was desirable that the history of the phenomena communicated by each observer of past observations of transits should be perused. Some injury had resulted to the observations in 1874 for want of such preparation. (Applause.)

The President: I am sure there will only be one opinion as to the great importance of this paper. It may perhaps be almost premature to invite any expressions of opinion in regard to what has been here stated; and I will only ask you to return your cordial thanks to the Astronomer-Royal for this most important paper. (Applause.) It has evidently been most carefully considered with regard to the results which were obtained from the last transit; and I hope that the next transit will give a more satisfactory value of the parallax than we were able to obtain from that of 1874.

Mr. Glaisher next read a paper by Professor Adams criticising the Astronomer-Royal's investigation of the theoretical value of the moon's mean motion, a description of which was given at the last meeting of the Society.

Mr. Glaisher also stated that another paper, wholly mathematical, and therefore unfit for reading to the Society, had been received from Professor Adams.

The Astronomer-Royal: For the reason I have already given I would rather not enter into the details of this question. It is extremely difficult, unless Professor Adams enters into the question term by term of my factorial table, to give an answer to his paper. I have no doubt I have secured accuracy; but Professor Adams, for instance, relies upon methods which proceed by powers of *m*, whilst I have not. Thus there is a total divergence of methods; and it would be impossible, without a discussion of some hours between ourselves, to make the comparison. I am entirely disposed to give my theory to the world, and to rely upon it. I do not wish to take up these things in any polemical spirit. I do not find fault with Professor Adams, and, if he finds fault with me, I shall not answer. (Applause.)

Mr. Neison: I think the matter is really too important to be left in any uncertainty. I looked at Professor Adams's paper, and I think that he asserts that the Astronomer-Royal has neglected certain terms that affect the moon's mean motion. That is of course a subject which only requires the attention of mathematicians to ascertain whether Professor Adams's view is right or not.

Mr. Freeman: I was not quite clear whether the Astronomer-Royal in his investigations had taken only the theoretical formulæ, and worked from these, or whether he had in any degree adopted the actual co-efficients. To mathematicians I should suppose that no result would be perfectly satisfactory which did not deal entirely with the literal forms of expression that occur in the representation of the disturbances of the moon. There would not be any check of a sufficient character one would fear on the accuracy of the final result, without going carefully through the combination of the expressions that are required for the investigations.

The Astronomer-Royal said he would explain the general principle. It was a principle, not of investigating any term complete, but of the alteration made in the value of terms by the alteration of some co-efficients. The quantity is never a large one. In working with a correction of any large co-efficient the magnitude of that co-efficient does not enter into the final operations, the process only defines the alterations to be made in it. These are to be determined by factors of the correction, and these factors have been prepared with the greatest care, and in them there was no such omissions as Professor Adams had pointed out. It was in his power to state these more definitely, but some of them were printed in rather a confused manner. If any member of the Society wished for more information he would be happy to give it in some way or other. He did not determine a co-efficient everywhere. He took co-efficients as computed by others and made the subject of his investigations, not the co-efficient itself, but the correction which was required to it, which was always a small quantity, and which never required higher powers than the first. In the method he had pursued such quantities as m and m^2 did not occur at all, and if anybody would take the pains of looking into them, he would be glad to give them every facility. As he had said, the printing of them was rather confused, and it might require a little alteration to supplement the manuscript, which he would be happy to do to the extent it could be necessary. He would add that the external factor of his result might require examination. (Applause.)

Mr. Neison then read a paper *On the determination of the Solar parallactic inequality in the motion of the moon.* He explained that the paper was a long one, and had been drawn up by Mr. Campbell and himself; he would, therefore, give a short account of the results. The parallactic inequality was a term in the moon's mean motion, and had been used for the purpose of determining the solar parallax, and if not for certain difficulties it would afford the most satisfactory method of determining the parallax, because the solar parallax was multiplied by a large co-efficient. So that if they could determine the value of the parallactic inequality to half a second of arc, they would be able to determine the value of the solar parallax to $\frac{1}{15}$th of that amount. Unfortunately when investigations have been made with the view of determining the parallactic inequality, it had been found that there are a number of difficulties in the way, which render the results obtained not so satisfactory as could be wished. In the present paper they had investigated all the results obtained previously, and had examined whether there might not be some imperfections in the methods adopted which would account for the discrepancies that existed.

The principal results were those of the Astronomer-Royal, Professor Hansen, Mr. Stone, and Professor Newcomb. With respect to the Astronomer-Royal's results, as far as the method employed was concerned, there was no objection which could be urged against it, but many of the observations used had been made at an early period—from 1750 to 1850—and there are certain small errors which would make the parallactic inequality too large for many years at a time. Unless these are corrected the results would be too large at certain periods and too small at others. The paper contained a full account of these errors. Nothing definite can be done with Professor Hansen's results, because he had nowhere published the method or the data he has used. Mr. Stone has published a very elaborate paper, and sent it to the Society. Unfortunately, however, only an abstract was printed, and the paper could not now be found, so that they were unable to criticise the details of his work. Sufficient had been published, however, to show that there are systematic errors involved in the result obtained by Mr. Stone. The observations he had used were founded upon the assumption that the places given by Hansen's tables would be the same at midnight as six hours before or after, but this was not the case, and there were large systematic errors introduced in this manner. With respect to Professor Newcomb's results there were also corrections needed, and again the theoretical tables employed proved to be of insufficient accuracy. They then proceeded to a fresh discussion of all the observations made at Greenwich, between 1862 and 1876, first of all determining the value of the parallactic inequality, on the assumptions of Professor Newcomb and Mr. Stone. They found the errors, which they had on theoretical grounds pointed out must exist, did exist, causing discrepancies amounting to 10″.

A great objection made to this method of determining parallax had been that the irradiation at the limb of the moon could not be properly estimated. The darker the sky the moon is seen against, the greater is the irradiation at the limb, but taking the value of the lunar diameter deduced from the observations themselves they had deduced as the true value of the solar parallax 8″·843, with a probable error of ±0″·007. This would be satisfactory. Further investigation, however, showed that owing to the influence of the planets upon the moon there would be small variation extending over a period of about 46 years. The Astronomer-Royal in one of his early papers pointed out the values he obtained seemed to be liable to an inequality of about 46 years, but he also said that such an inequality seemed extremely unlikely. Later investigations had enabled them to see that there were such inequalities. They could not find one having a period of 46 years, but they had found one of 48 years and another of 44 years, and the results were so very close to the hypothesis that it was likely that one of these produced this variation pointed out by Sir George Airy. This had been discovered by them quite independently of the Astronomer-Royal. They then considered what would be the effect of this inequality. They first showed that when the results obtained by the different investigators were compared they did not seem to accord. When all the values were compared the value of the parallactic inequality became 124″·53 according to Sir George Airy, 123″·84 according to Mr. Stone, and 124″·42 according to Professor Newcomb, whilst their value was 125″·03. Supposing they applied the correction above referred to, for the inequality with the 45 years period, they found that their results were brought into more complete accord. They then became 124″·13, 124″·01, 123″·96, and 124″·03, which it would be noticed did not differ by more than $\frac{1}{10}$th of a second. By this they concluded if such a term really exists the solar parallax must be 8″·773, or very nearly the same as that obtained by Mr. Gill from his observations of Mars. The plan hitherto pursued for determining the moon's place had been to determine the place of the moon's limb and deduce the place of the moon's centre, but this involved the difficulty as to the correction to be employed for irradiation, and Mr. Campbell had therefore undertaken at the Arkley Observatory a series of observations of transits of a small crater or spot near the centre of the moon, they thus got rid of all the variations in the semi-diameter. The effect of libration had of course to be taken into account, but this could be very satisfactorily done. (Applause.)

The Astronomer-Royal: I have always been a little unwilling to enter into the determination of the solar parallax from the parallactic inequality, for I consider it subject to much doubt, from the circumstance that the great majority of observations of the moon are taken between six hours on one side of the meridian and six hours on the other side, and in both cases the parallactic inequality and the variation combine with the same sign and cannot be determined.

Mr. Stone remarked that Mr. Neison had made no reference to the term depending upon three times the difference of the mean longitudes of the moon and sun, although this term was most important in discussions of observations for the parallatic inequality, and although different values had been adopted in the different lunar tables. This point must be cleared up before a comparison of the values obtained could be made. In the second place, he could not conceive how Mr. Neison had got so small a probable error for his determination of the parallax. The determination of the semi-diameter of the moon was a point of very great difficulty indeed. He was afraid Mr. Neison must have deceived himself with regard to the difficulties of this irradiation question. He certainly had not touched the real difficulties in my paper on the moon's semi-diameter. With regard to his own result, it was very similar to Mr. Neison's, so close that he would hardly care to say what the difference might be. But at the time he brought it forward he felt a great difficulty in knowing what value of the semi-diameter to adopt.

Mr. Neison: First, as to the empirical correction for the semi-diameter which has been referred to by Mr. Stone. No such correction is used in the present investigations. The value for the semi-diameter and the irradiation are rigidly determined from the observations. Sir George Airy referred to the variation and the parallactic inequality varying together; that of course is perfectly true, but when the paper is printed it will be seen that we completely get rid of any error due to it. We take into account and determine independently both the variation and the parallactic inequality, so that as far as any error on that account is concerned, the results are quite free from them.

A vote of thanks was accorded to Mr. Neison.

The President said: The last comet discovered by Mr. Schäberle was of somewhat unusual interest, because it was discovered early in its appearance, and from what could be seen it was not unlikely we may see it till January next. There was thus an excellent opportunity of working out its orbit, although the perihelion distance was not less than 1·5 of the earth's distance. The comet would not arrive at its least distance until Nov. 3rd. He had been greatly assisted in getting out these elements, which seemed to be unusually precise, by an observation made by Major Tupman on the 8th of the present month; Major Tupman being fortunate enough to witness the passage of the comet by a star well determined by Argelander. It passed within 45 seconds of arc of the star, and the angular position altered during Major Tupman's observation by something like 100 degrees. He referred to the comet of 1695, which was supposed in the year 1843 to be identical with the comet of that year.

The observations were first discussed by Burckhardt; but he found that Burckhardt's elements exhibited large errors, and that the orbit had a great similarity to that of the comet of 1843, and, therefore, to that of this year. Mr. Christie had read a letter from him at the last meeting, in which he had expressed an opposite view; but he now thought, from the work he had gone through since then, that he must qualify what he had then said, and that the comet of 1695 might be identical with this great southern comet.

Mr. Christie: What is the opinion of the President as to the comet of 1689? Three orbits have been given, which differ, and the true elements might perhaps agree with this comet.

The President: That comet was another of those discussed much at the time of the great comet of 1843; and I asked a former colleague, Dr. E. Vogel, to re-discuss the observations, and he obtained an orbit which differed entirely; but on looking into it lately, there is reason to think the elements require further examination. At the same time I do not think there is the probability of that comet coming into competition with the comet of 1843. There is another, in 1618, of which we know but little, which appeared under the same circumstances as those of 1843 and 1880. It became suddenly visible with a long tail; the nucleus was lost in the solar rays, and in a very few days the comet vanished. These have been peculiarities of all these comets; but observations of the comet of 1618 have not been discovered, as far as I know. (Applause.)

Mr. Downing next read a paper *On the possible ten-monthly period of variation in latitude.* From an examination of Greenwich observations, he came to the conclusion that there was a variation, which only amounted to a small fraction of a second, in the altitude of stars, which could be accounted for on the supposition that the earth's axis of rotation describes a small circle in the heavens in a period of ten months.

Mr. Neison said: As far as I understood the paper, it meant that the north pole of the earth is moving round the real axis of rotation in an orbit of about 6 feet in diameter, and taking ten months to move round it. Such a movement is probably caused by the figure of the earth not being symmetrical with regard to its axis of rotation. I would ask whether this investigation has not been made for other periods? I do not know whether the subject has been properly investigated. Perhaps Professor Cayley can tell us?

Professor Cayley said, he had not given the subject his attention.

Mr. Christie: I think we must be pleased to find the latitude is subject to so small a variation, as far as we can trust this result; and if we really find the distance of the pole of rotation is determined within a foot or so, I do not think there is much to complain of.

Mr. Stone remarked that some years ago he had investigated the question as to whether there was any periodical variation in latitude, and he came to the conclusion that there was, but could not recollect if it was the same as that found by Mr. Downing, but it was some quantity equally small.

Mr. Christie read a paper, by Dr. H. Draper, *On a photograph of Jupiter's spectrum, showing evidence of intrinsic light from that planet.* A small photograph was handed about the meeting, showing the spectrum of Jupiter in juxtaposition with the spectrum of a small part of the moon. The spectrum of Jupiter was derived from light passing through a slit placed across the planet's disc from north to south. At one end of the spectrum the light appeared to be brighter towards the centre of the planet, while at the other end of the spectrum the light was decidedly brighter towards the top and bottom edges of the spectrum. The adjacent lunar spectrum was, however, of uniform brightness across its whole breadth.

Mr. Ranyard said: I should have been glad if Dr. Draper had given some more details with regard to the instrument and the length of the slit employed in taking this photograph. From a cursory examination I am inclined to suspect that possibly the breadth of the spectrum does not correspond with the breadth of the planet's disc; the edges of the planet's spectrum appear as sharply defined as the edges of the lunar spectrum, and it is possible that the light of the planet was admitted through the little shutter in the slit, similar to the one that must have been made use of to obtain the lunar spectrum. If so, it does not follow that the breadth of the planet's spectrum extends from pole to pole; and the peculiarities of the spectrum may only be due to the absorption of the equatorial belts. Before drawing any great physical deduction, such as Dr. Draper draws from this photograph, I should like to know whether it was taken with a reflector or a refractor, and whether the slit was in focus, and for what part of the spectrum; though I do not understand how the slit not being in focus could produce the defect of light towards the centre at one end of the spectrum; and it is also difficult to understand how such an effect could be due to photographic irradiation.

Mr. Christie: As regards the details given in the paper nothing is mentioned as to the length of the slit, the only thing mentioned is that there was an exposure of fifty minutes given, and that the photograph was taken with a telescope of 183 inches focal length. I conclude it was with Dr. Draper's refractor, and not his reflector, but it is not expressly stated in the paper, though it is an important point.

Capt. Abney said: I should hesitate very much before accepting Dr. Draper's conclusion that this is due to Jupiter's emitted light visible in the photographic spectrum. When you have a body emitting light the first part of the spectrum emitted are the rays of lower refrangibility, and not the higher. The rays of light are those somewhere between little *b* and F. In using a refractor, if the slit is out of focus for any ray, there is found to be a greater intensity of light in one part of the spectrum than in another. He would hesitate very much indeed, therefore, before he admitted Dr. Draper's views that the photograph showed evidence of emitted light. He believed the phenomenon was principally due to photographic defects.

Mr. Christie remarked that Dr. Draper explained in his paper on photographs of stellar spectra that one of the incidental advantages of his instrument was that the rays were not in focus throughout the spectrum. He relied greatly upon this want of perfect achromatism as an advantage, and the photograph now shown was, Mr. Christie thought, taken under the same conditions as those of stellar spectra.

Capt. Abney added that he had found much the same anomaly in photographing the sun itself. It had nothing to do with the spectrum, but was a photographic defect. He should at once have said that it was a photographic defect, and that if a number had been taken very likely that phenomenon would not have been discovered in all.

Mr. Ranyard: The defect of light at the centre of the spectrum cannot, I think, be due to over-exposure—sometimes spoken of as solarization—for that would be first shown, not at one end of the spectrum, but at the brightest region near G. With a refracting telescope, however carefully the object-glass may be corrected for photography, all the rays of the spectrum will not be in focus on the slit, and where they are not in focus it is easy to understand how the central part of the spectrum may be more acted upon by light than through the top and bottom of the slit.

Mr. Brett: The shadows of Jupiter's satellites are usually said to be black, and though I do not pretend to assume that they do *not* emit rays of light, I presume that the plate must be remarkably sensitive that recorded any spectrum emitted by Jupiter under the shadow of the satellite.

Captain Abney: I beg to state there is no such thing as solarization. Theoretically there is no such thing. I alluded to optical photographic defects, not to solarization. If it were solarized it would be about the G line.

Mr. Ranyard: Can you explain how the edges of the spectrum are more intensely acted upon than the centre in the more refrangible rays?

Captain Abney: I have produced the same effect with the solar spectrum about the edges, and not about G. It is want of definition.

Mr. Christie: Anyone who examines the spectrum of stars will see it runs off in a trumpet-shape towards the violet end, and that might account for one part of the phenomena, *i.e.*, the two edges being bright. The other part may be accounted for otherwise in the intrinsic brightness of the equatorial parts of Jupiter.

Mr. Bosanquet then explained the use of two precessional globes referred to in his previous communication. Some time since when working out points connected with ancient Babylonian and Assyrian astronomy, he had made these globes, thinking they were the first of the kind; on coming to the Society he had, however, found a globe of the same sort in the library. Mr. Bosanquet then showed, by means of his globe, that in the time of Homer, the stars of the Great Bear would always have been above the horizon, as observed from the latitudes where Homer is supposed to have written.

Mr. Christie: There is also a similar globe at the Greenwich Observatory, which the Astronomer-Royal had made 30 or 40 years ago. We have found it useful when we are troubled with those Egyptian and Babylonian questions. But I am very pleased to think that Mr. Bosanquet has made a globe like this, for now people will apply to him to answer these points. (Laughter.)

A vote of thanks was then accorded to Mr. Bosanquet.

The President: I think I ought not to allow the members to separate without mentioning a most serious loss to the astronomical world in the death of Professor Peters. He died after a long illness on the evening of Saturday last, and I think it is only right that the meeting should not part without some expression of regret for the loss of so eminent an astronomer.

Lord Lindsay thereupon moved that a vote of sympathy should be conveyed to the widow of Professor Peters.

This was seconded by Mr. Marth and carried.

Lord Lindsay: Before concluding the business might I ask you if any reliable information has come to us as to the southern comet?

The President: None from the Cape, but the observations from Melbourne were published in the last *Monthly Notices*. The best series of observations we have are those made by Dr. Gould, and I think we shall have to rely mainly on them for the determinations of the orbit.

The following papers were also laid on the table and partly read:

J. Tebbutt: *On the variability of B.A.C.* 2472.

J. Tebbutt: *On the longitude of the Observatory, Windsor, N. S. Wales.*

J. Tebbutt: *Notice of the great southern comet.*

D. W. Barker: *Observations of the great southern comet.*

H. T. Vivian: *Elements of the great southern comet* 1880.

D. P. Todd: *On a mechanical attachment for equatorial mountings to facilitate sweeping in Right Ascension.*

Lord Lindsay: *Observations of comet* 1880 *b* (*Schäberle*) *made at Dun Echt with the filar micrometer of the* 15*-inch refractor.*

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

MARS.

(Translation.)

Dear Sir,—Concerning the interesting letter of Mr. Burton in the last number of the *Astronomical Register,* I have the honour to send you a new Memoir on the planet Mars, which I shall be glad to forward to any astronomers interested in the subject who may favour me with their correspondence. It fully bears out the opinion of my excellent correspondent, Mr. Burton, and shows that a large number of details hitherto inexplicable in the drawings of Mars, receive their natural interpretation by the presence of the *canals* of M. Schiaparelli. We recognise, either entirely or in part at various epochs, *Hiddekel, Triton, Nepenthes, Xanthus, Scamander, Cyclopum, Læstrigonum, Nilus, Fluvius, Oceanus, Gihon, Indus, Hydaspes, Ganges, Eosphorus, Araxes, Agathodæmon, Herculis Columnæ, Sirenum, Titanum,* or *Gigantum,* and *Eunostos.* Mr. Burton is quite right in insisting on the existence, already some time recognised, of *Dawes' Strait* (Hydaspes), and of *Huggins' Inlet* (Cyclopum). Mr. Dawes also represented *Bessel Inlet,* which M. Schiaparelli re-discovered in 1879, as he was good enough to inform me. *Nasmyth Inlet* was also known since Keenowski. The tendency of the seas of Mars to stretch out like the *necks of bottles* was recognised as a distinctive and singular characteristic of that planet. In the present day we see by the admirable labours of M. Schiaparelli that characteristic more and more confirmed. I own that I cannot comprehend the incredulity with which these results have been received, and the astonishment inspired by them. I am convinced that the detailed memoir of Mr. Burton will afford us fresh arguments in favour of that opinion, if I may judge by the very important chart of which he has obligingly sent me a copy. In fact, the chart contains various canals of which even a superficial examination enables us to a certain point to identify as those of M. Schiaparelli. I pray you, dear sir, to accept the assurance of my best regards.

Louvain (Belgique): May 5, 1880. Dr. F. TERBY.

I shall be delighted if this letter should interest your readers.

THE "CANALS" OF MARS.

Dear Sir,—In reply to the courteous letter on this subject by Mr. Burton, in your last number, I would remark that I have evidently been very unfortunate in my opportunities of seeing these "canals." At Madeira during the best portion of the opposition of 1877 they were not visible. To Prof. Schiaparelli they only became visible at a later period, after I had left the island, and during the past opposition the London air was particularly unfavourable to the exhibition of minute markings. I do not remember a single occasion when it was both clear and steady. The best views were on foggy evenings—but generally the definition was brilliant and unsteady.

But after Mr. Burton's exact description of these forms, and the characters of their appearance, it would be impossible to doubt their existence—and it would be interesting and instructive—if not satisfactory, to make a collection of all the marks of this character that have been seen by various observers, and lay them down on a chart for future guidance.

There is one point in connection with this discussion of no small importance to those who are engaged in the study of Mars, and that is, the duty of regarding with respect the observations and drawings that may differ from their own. There is a great tendency to question the existence of any form that has not been personally observed; but when we consider the disappearance and re-appearance of such a definite mark as the Dawes Sea, of Proctor, and especially the various appearances presented to the same observers by the same portion of the planet's surface, on different, and it may be consecutive, nights, we should be prepared to give a generous amount of credence even to those drawings that seem to contradict each other.

I am, dear sir,
Yours very respectfully,
NATHL. E. GREEN.

THE CRATER ARCHIMEDES.

While observing this crater since last November, considerable interest has been caused by the appearance of the craterlet on the north-east portion of the floor and near the peak ρ. Twenty-five observations have been made of it; thirteen by Mr. F. B. Allison, of Chesterfield, the remainder by myself. Once Mr. Allison described it as a rock mass, and once as a small hillock. Seven times it has appeared to me as a minute, bright, and apparently isolated spot; on two occasions I was unable to decide whether or not it was joined to the wall. In addition to this it has frequently been seen as a projection from the wall; this being an important point, I will give the observations in full:—

d.	h.	m.	
1879. Dec.			
24	8	0	Mr. Allison writes, "it appeared as a distinct, bright point, which did not look like a crater, but might be a projection from the wall."
1880. Feb.			
19	8	0	"A small projection at north-east corner was seen like a spur from the wall in the position of this craterlet," by Mr. Allison.
20	11	30	"A little projection in the north-east, seen well as a small spur from the wall; it did not show any signs of being a crater although the details of the neighbourhood were exceedingly plain."—Mr. Allison.
21	9	30	Mr. Allison.—"The small projection at north-east distinct." On the same date I recorded the craterlet distinctly visible, though appearing more as a white projection.
Mar.			
19	8	0	Mr. Allison saw the projection from the wall in the north-east corner.
21	9	30	Mr. Allison recorded, "the point of the little promontory in the north-east corner very bright, not distinguishable as a crater." The same appearance was noticed by me on 22d. 10h. 30m., and by Mr. Allison on 23d. 8h. 30m. and on 24d. 9h.
Apl.			
20	9	0	I noted that it was joined to the wall by a bright, narrow streak.

It is remarkable that it has never been seen as a crater; and, while it is easy to understand its looking like a rock or hillock, (I have noticed the same with Messier *f*), it is difficult to explain its appearing as a projection. Mr. Neison's map shows no projection from the wall. Mr. Knott has drawn a projection, although apparently of a different kind from that mentioned above; this is, however, not quite certain. That a projection has been observed is evident, but anything more must be proved by future observations.

Mr. Allison's instrument is a 4½-inch refractor; mine an 8-inch reflector; usual powers from 100 to 200.

Bedford: May 8, 1880. T. P. GRAY.

OCCULTATION OF ζ CANCRI.

Sir,—The appulse to ζ Cancri of the moon at Greenwich on the night of the 17th, proved an occultation in more westerly parts of England, and the duplicity of the star rendered it a somewhat interesting phenomenon. Every observation of this sort is more noteworthy at the present time, as there is no occultation brighter than a third mag. star visible in this country for some years to come. On the occasion referred to, as seen with power of only 70 on 2¼-in., too small to separate the pair, the impression conveyed was that the star disappeared at two distinct instants, first diminishing on a sudden to one of lesser magnitude, and then vanishing. Final disappearance 10h. 49m. 21s. Estimated interval four seconds. At 11h. 2m. 51s. the emersion from the bright limb took place, when, doubtless from the smallness of the instrument, the phenomenon of double appearance was not noticed.

Abbenhall Rectory, Faithfully yours,
Gloucester: April 28. S. J. JOHNSON, F.R.A.S.

40 *LEONIS MINORIS.*

I have recently discovered a very faint attendant to this star, the position of which by two good measures is:

$$P = 122^{\circ}\cdot1. \qquad D\ 10''\cdot76. \qquad 1880\cdot27.$$

The companion I estimated to be of the thirteenth magnitude of Struve's scale.

Chicago: April 17, 1880. S. W. BURNHAM.

SOLAR PHYSICS.

We reprint the following letter from Lord Lindsay which appeared in the *Daily Telegraph*, Wednesday, May the 19th:

Sir,—With reference to the paragraph on Solar Physics in your issue of to-day, recommending that observations of sun spots should be made at the national expense, for the purpose of predicting the character of the seasons, I would point out that observations of sun spots have for years been made at observatories, both public and private, too numerous to mention. Records of observations of sun spots from the date of the invention of the telescope down to the present time have been collected by Dr. Wolf of Zurich, who has deduced from them tables showing that sun spots do not vary in the regular manner that is commonly supposed. On one occasion more than sixteen years elapsed between two consecutive periods of maximum sun spot development, and at another time two consecutive maximum periods followed one another within seven and a half years.

We are therefore far from being able to predict the dates when sun spots will be most developed; and, if men were able to make such predictions, there is no evidence to show that there is any more connection between sun spots and famines than there is between comets and wars, or the planets and the death of kings. That such a reason should be given for founding a new physical observatory, at the national expense, shows that the promoters of the undertaking are appealing for support to an unscientific class. It seems beneath the dignity of science to hold out to the public such promises. An observatory, if established under the patronage of those who can be imposed upon by sun spot astrology, is not likely to be efficiently controlled. I feel, however, sure that physical astronomy both can and will prosper, untrammelled with any additional national aid.

I am, sir, your obedient servant,
LINDSAY, Foreign Secretary,
Royal Astronomical Society.

47, Brook Street.

THE DOUBLE STAR SOUTH 390.

This object was discovered by South in 1824. It is thus described by Sir James in the Philosophical Transactions, 1826, Part I.: "Double; 9th and 10th magnitudes; both bluish, and do not bear a good illumination."

It was next measured by H_2 in 1830, and several times by the same observer at the Cape. Secchi was the next to take it up, and in 1877 the observers at the Cincinnati Observatory made some measures of it. The results as hitherto presented run thus:

°	″		
32·9	7·78	1824·9	South.
34·4	10 (estd.)	30·8	H_2
32·0	6·66	35·8	"
36·9	6·66	35·8	"
32·7	6·65	37·8	"
86·8	6·43	55·9	Sa.
214·9	6·36	77·8	C. O.

For the following note I am indebted to Mr. H. Sadler, who has carefully examined Sir John Herschel's MSS. and also Secchi's individual measures.

H_2 has the following note in his MSS.:—"Unless some grand mistake, a most wonderful binary star." Doubtless Sir John was led to write these words by the great angular change which Secchi's measure indicated—an increase of some 50° in about twenty years. The "grand mistake" arose thus:—On page 117 of the *Catalogo delle Stelle Doppie*, Secchi gives 88°·62, 1855·950 and 85°·10 in 1855·955. On page 70 of the *Riduzione delle* *misure* in the *Descrizione del Nuovo Osservatorio* Secchi has 86°·86, 1855·953, *i.e.*, the mean of the results in the *Catalogo.* But on referring to the *separate* measures on page 48 of the *Descrizione* Mr. Sadler found the following:—

No. 227. Anon., *h*1138, 1855·950; 33°·17; 6″·359.
No. 228. χ^1 Balena, Σ. 147, 1855·950; 88°·62; 3″·64: mag. 5, 7.

Hence it is evident that Secchi has given to No. 227 (*i.e.*, South 390) the angle which belongs to No. 228. Once more collecting the measures and subtracting 180° from the last angle, we have the true history of this star. And it also clearly appears that it can no longer be regarded as a binary in rapid motion.

°	″		
32·9	7·78	1824·9	So.
34·4	10 (estd.)	30·8	H,
32·0	6·66	35·8	”
36·9	6·66	35·8	”
32·7	6·65	37·8	”
33·2	6·36	55·9	Se.
34·2	6·36	77·8	C. O.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax. JOSEPH GLEDHILL.

DUN ECHT CIRCULAR, No. 5.

Professor George Forbes, of Glasgow, has reason, from a study of the action of planets on comets, to believe that a planet exists at present in

R.A. ... 11h. 40m.
N.P.D. ... 87°

or within perhaps 5° or so of that spot, close to the ecliptic, that the opposition is just past, and that its distance from the sun is 100 times that of the earth.

Any astronomer wishing to search for the planet can get a copy of Professor Forbes' memoir on the subject (printed, but not yet published) by applying to him at Anderson's College, Glasgow.

Dun Echt Observatory:
1880, April 29.

DUN ECHT CIRCULAR, No. 6.

Mr. Baxendell communicates the following observations of three new variables:

a, 1880, Jan. 28, 8·7m. star, 37·5 sec. *p* and 7′·3 *n* of D. M. + 13°, 1655; reddish; in a cluster of seven or eight very small stars,
About the middle of March about 1m. fainter.
May 4, 12·9m.

b, 1880, Mar. 12, 9·4m. star, 47·2 sec. *p* and 0·4 *s* of D. M. + 18°, 2955; very slightly reddish.
Maximum = 9·2m. about March 19.
May 8, 11·8m.

e, 1868, July, D. M. + 35°, 2892 (9·0m. in D. M.) invisible with 5-in. achromatic.

1880, May 3, 0·3m. less than D. M. + 35°, 2890 (9·4m. in D. M.).

Dun Echt Observatory:
1880, May 14.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JUNE, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Archimedes.—A very interesting series of observations of the streaks on the floor of this walled plain have been made between 1879, November 28, and 1880, March 29, by Mr. T. P. Gray, of Bedford, and Mr. F. B. Allison, of Chesterfield. This walled plain is nearly fifty miles in diameter, the floor being, as described by Mädler, mirror-like without any hills or ridges; it is, however, divided into seven streaks or zones of unequal brightness. It is these streaks or zones that have been observed by Messrs. Gray and Allison, and to which we desire to solicit the attention of the observer.

The number of observations made by these gentlemen is 34, including 12 separate drawings. Among these drawings is a key map, on which *all* the streaks seen at various times, but not simultaneously, by both observers are depicted; also another key map containing those streaks only which have been seen several times and *by both observers*. It is considered that the existence and positions of these streaks are to some extent fixed.

Commencing from the S. wall at one-fourth of the transverse diameter from S. to N. we have, as seen by both gentlemen, the S. edge of a *bright* streak, this streak is connected with the S. wall by a short bright streak at right angles to the streak just mentioned, which Mr. Gray describes as slightly curved, running from the W. to the E. wall, and being about parallel to the S. wall. Mr. Gray says, "it is almost always visible, having been observed 28 times by both observers.

The central bright streak is very interesting, it runs right across the centre of Archimedes from W. to E., it has been observed 30 times by both observers, the following are a few of these observations:—1879, November 30d. 16h., moon's age 17·2 days. **Gray**, "more distinct than the other streaks." 1879, December 24d. 8h., moon's age 11·3 days. **Allison**, "visible brighter at the W. end, on it was a dark oval area to east of, but contiguous to centre. 1880, February 20d. 11h. 30m., moon's age 10·5 days. **Allison**, streaks, and oval spots on it, observed. 1880, March 24d. 11h., moon's age 13·9 days. **Gray**, streak seen central dark patch, large, oval, and distinct. 1880, March 28d. 14h. 30m., moon's age 18·1 days. **Gray**, visible with central dark patch under the E. side of the short connecting streak with the S. wall. The existence of the central dark patch is supported by seven observations by Mr. Allison and ten by Mr. Gray.

In addition to the streaks on the floor, Mr. Knott has seen as many as ten small craterlets within the border of Archimedes. Gentlemen desirous of taking part in the observations of the streaks and craterlets on the floor may be supplied with plans of each by sending stamped and addressed envelopes to the writer at the address below. It may be as well to state that it is important to obtain a correct outline of the wall in which are depicted the promontories or bays indenting it.

2, Percy Terrace, Cedars Road,
Water Lane, Stratford: May 5, 1880.

ASTRONOMICAL OCCURRENCES FOR JUNE, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Antares.
Tues	1	13 18 14 1	Occultation of 45 Piscium (6) Reappearance of ditto	1st Oc. R.	14 2	— 11 38·8
Wed	2	0 6	Superior conjunction of Mercury and Sun Conjunction of Moon and Jupiter 6° 55′ S.	3rd Oc. R.	14 40	11 34·9
Thur	3	7	Conjunction of Moon and Saturn 7° 44′ S. Saturn's Ring: Major axis=37″·49 Minor axis= 9″·40			11 30·9
Fri	4					11 27·0
Sat	5					11 23·1
Sun	6	13	Conjunction of Moon and Venus 3° 12′ S.			11 19·1
Mon	7	9 55	● New Moon	1st Sh. I.	15 21	11 15·2
Tues	8	8	Conjunction of Moon and Mercury 0° 32′ N.			11 11·3
Wed	9			1st Tr. E. 3rd Ec. R.	13 20 13 48 33	11 7·3
Thur	10			2nd Ec. D.	14 45 26	11 3·4
Fri	11	15	Conjunction of Moon and Mars 4° 12′ N.			10 59·5
Sat	12			2nd Tr. E.	14 17	10 55·5
Sun	13					10 51·6
Mon	14					10 47·7
Tues	15	9 51	☽ Moon's First Quarter Illuminated portion of disc of Venus=0·991 Illuminated portion of disc of Mars=0·954	1st Ec. D.	14 25 22	10 43·7
Wed	16			1st Tr. I. 1st Sh. E. 1st Ec. D. 1st Tr. E.	13 3 13 59 13 31 6 15 17	10 39·8

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Antares.
Thur	17					10 35·9
Fri	18					10 31·9
Sat	19			2nd Tr. I. 2nd Sh. E.	14 15 14 16	10 28·0
Sun	20	13 3 13 36	Occultation of 22 Scorpii (5) Reappearance of ditto	3rd Tr. E.	13 25	10 24·1
Mon	21					10 20·1
Tues	22	1 45 8 42 11 27 12 32	○ Full Moon Occultation reappearance of 24 Sagittarii (6) Occultation of 26 Sagittarii (6) Reappearance of ditto Eclipse of the Moon invisible at Greenwich			10 16·2
Wed	23		Saturn's Ring: Major axis=38″·56 Minor axis=10″·06	1st Sh. I. 1st Tr. I.	13 37 14 59	10 12·3
Thur	24			4th Sh. I. 1st Oc. R. 4th Sh. E.	13 15 14 22 14 24	10 8·3
Fri	25					10 4·4
Sat	26	15 12 15 35	Occultation of κ Aquarii (5) Reappearance of ditto	2nd Sh. I.	14 7	10 0·5
Sun	27	14 32 15 45 14 36 15 43	Occultation of 9 Piscium (6) Reappearance of ditto Occultation of κ Piscium (4½) Reappearance of ditto	3rd Sh. E. 3rd Tr. I.	12 8 14 58	9 56·5
Mon	28	21 57	☾ Moon's Last Quarter	2nd Oc. R.	14 44	9 52·6
Tues	29	21	Conjunction of Moon and Jupiter 6° 59′ S.			9 48·7
Wed	30	17	Conjunction of Moon and Saturn 7° 46′ S.	1st Sh. I.	15 32	9 44·7
JULY Thur	1	12 48	Occultation reappearance of B.A.C. 782 (6)	1st Ec. D.	14 41 42	9 40·8

THE PLANETS FOR JUNE.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	4 42 43	N.22 58½	5″·0	23 57·4
	9th	5 49 1	N.25 3½	5″·2	0 36·0
	17th	6 58 58	N.24 46	5″·6	1 14·3
	25th	7 56 46	N.22 22	6″·6	1 40·5
Venus ...	1st	3 55 37	N.19 37	9″·8	23 10·4
	9th	4 36 41	N.21 37½	9″·8	23 19·9
	17th	5 18 44	N.22 59½	9″·6	23 30·4
	25th	6 1 27	N.23 39	9″·6	23 41·6
Jupiter ...	3rd	0 50 53	N. 4 9	34″·4	19 58·3
	11th	0 53 49	N. 4 38½	35″·2	19 31·8
	21st	1 1 20	N. 5 10½	36″·2	18 58·0
Mars ...	15th	1 41 44	N. 8 1	15″·4	20 1·8
	23rd	1 44 10	N. 8 13	15″·4	19 32·8
Uranus ...	1st	10 28 40	N.10 23	4″·0	5 46·3
	17th	10 30 9	N.10 13½	4″·0	4 44·9

Mercury sets with the sun, at the beginning of the month—afterwards before sunset, the interval gradually increasing to about an hour and a half on the last day.

Venus is too near the sun to be well observed.

Jupiter may be observed two hours and a half before sunrise at the beginning of the month, the interval increasing.

Saturn rises on the 1st an hour and a half before the sun, the interval increasing.

LESSONS IN SELENOGRAPHY.

By the Rev. W. J. B. Richards, F.R.A.S., and W. R. Birt, F.R.A.S.

Chapter II.—Section III.

Ring Formations or Circular Mountains. Having treated on mountains, both isolated and in ranges, we pass on to the consideration of, perhaps, the most remarkable and characteristic class of mountains on the surface of the moon; many of these circular ranges of mountains being of so large a size as to form conspicuous objects even in an ordinary opera glass. They are divided, according to their size and structure, into the following classes, of each of which we propose to *describe* an example for the help of the student:—

1. *Walled Plains*, from 40 to 150 miles in diameter.
2. *Mountain Rings*, somewhat smaller, and inclosed by less intricate mountains.
3. *Ring Plains*, from 20 to 60 miles in diameter, inclosed by a single wall.
4. *Craters*, from 4 to 10 miles in diameter, usually with bright interiors.
5. *Craterlets*, from half a mile to 5 or 6 miles in diameter, often very bright.
6. *Crater Pits*, resembling craterlets, but very shallow and not so bright.

Copernicus. (109 in our map.) The most striking, and we may say imposing, example of this class of lunar formations is *Copernicus*, a most gorgeous spectacle when seen at sunrise or sunset. Its magnificent wall, when seen under either of these circumstances, presents the appearance of a circlet of pearls, owing to a number of peaks—perhaps fifty—that reflect the earliest and latest rays of the sun, as they emerge from, or plunge into the darkness of night, which forms a dark background to the wall under these circumstances. The wall is much terraced, and steep towards the interior, rising on the east to 10,896 feet, and on the west to 11,279 feet. Near it, shortly after sunrise, is a curious shadow, as of a peak projecting over the interior. Descending from the summit to the floor, after noticing the terraces finely illuminated by the early sun—which are well shown in an exquisite drawing by the late Sir John Herschel, reproduced in Plate V. of the fourth edition of his "Outlines of Astronomy"—the student finds it presenting a rough appearance, with a group of mountains situated near the centre.

The surroundings of *Copernicus* are more or less remarkable. The crater 108 on our map is named *Stadius*, which has so low a ring that it is discerned with difficulty under a high light. The crater marked 110 on our map, called *Eratosthemes*, is connécted with *Stadius* by a steep mountain, according to Webb 4,500 feet high. In the space between these three craters a large number of minute craters are seen; it has been considered that as many as 300 exist. They form a fine study; several are arranged in rows. They are best seen when the sun is rising on *Copernicus*.

Copernicus is the central portion of a mass of complex mountains and ridges, most of them radiating from the great crater, especially in its immediate neighbourhood; but at some distance from it ridges are seen more or less parallel with the walls, often crossing the radiating ridges. Under a nearly vertical illumination, not only is *Copernicus* the centre of a vast system of mountains, but it is also the centre of a remarkable cloud of light streaks, which extend from it on all sides, and appear to indicate that the formation or production of *Copernicus* was attended with immense disturbance of the immediate and surrounding regions. There are but few regions similar to them on the moon, and none that demand closer study, more careful observation, or greater system in the registration of the phenomena observed.

Many portions of the walls of the large inclosed plains often form continuous lines of cliffs bordering extensive tracts of high land, which occur on various parts of the moon. We remember tracing such a line of cliffs from the north-east border of *Albategnius*, No. 50 on our map, to two craters named *Sabine* and *Ritter*, which are not shown on our map. This line of cliffs passes near to two craters named *Halley* and *Hind*, which are situated near to the somewhat large and irregular formation known as *Hipparchus*, No. 51. *Sabine* and *Ritter* are shown on Neison's map, XXI.

The predominant number of circular mountain ranges consist of such as surround plains, and vary from immense inclosures to small craters inclosing a very contracted surface. As an additional example of a large plain, we select the formation *Petavius*, No. 2 on our map, situated on the meridian of 60° W., not only as a fine specimen of a large walled plain, which, were it situated near the central part of the moon, would appear to great advantage; but the western part, being within 30° of the limb, exhibits the phenomenon of foreshortening, the eye viewing the surface obliquely in consequence of the rotundity of the moon, so that a circular formation assumes the appearance of a somewhat elongated ellipse when situated near either limb. The interior of *Petavius* is convex, the height of the central portion being as much as 800 feet above the surface close to the walls; it contains a very considerable number of low hills and ridges and a fine central peak which attains an elevation of nearly 5,600 feet above the surface on the east. From this peak a fine rill or cleft extends in a south-east direction to the east wall. Neison shows, on map XIX., this cleft as cutting through the wall, a feature which every student should make himself familiar with; especially as Neison says the cleft can be seen with an aperture of two inches, we would recommend the beginner to look for it. Neison mentions two other clefts upon the floor of *Petavius*, one situated on the north, the other on the west of the central peak.

The mountains surrounding *Petavius* are steep and lofty, rising on the east to nearly eleven thousand feet, and in many places, according to Neison, exhibit portions of an interior wall.

Observations of *Petavius* should be commenced when the moon is about three days old, and, if possible, when the rays of the sun fall on the summits of the mountains of the western border, which, according to Neison, rises to 6,433 feet above the interior. The extent of shadow thrown across the floor should be particularly noticed, as also especially the time, to the nearest minute, at which the border of the shadow touches (as it recedes westwards at sunrise, or extends eastwards at sunset) the objects visible on the floor. It would be well to observe the aspect of the interior as the luni-solar day advances or declines until the lengthening shadows warn the observer, about three days after full moon, that *Petavius* is passing into the darkness of night.

(*To be continued.*)

Erratum.—In May number, page 101, line 11, for *Mr. Jackson Gwilt* read *Mrs. H. Jackson-Gwilt.*

Books received.—Professor Langley's Address (Section A) before the American Association for the Advancement of Science. Salem. 1879.—Measures of the polar and equatorial diameters of Mars. By Professor C. A. Young.—Method of investigating errors produced by irregularities in the form of the pivots of a meridian instrument.

ASTRONOMICAL REGISTER.—Subscriptions received by the Editor.

To March, 1880.

Jackson-Gwilt, Mrs. H.
Locke, W.
Stothard, Dr

To May, 1880.

Lawton, W.

To June, 1880.

Lewis, H. K.
Watson, J.

To July, 1880.

Denning, F. W.
Kelly, W. P.

To Dec., 1880.

Remington, Major
Warriner, H.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The Astronomical Register.

No. 211. JULY. 1880.

ROYAL ASTRONOMICAL SOCIETY.

Session 1880—81.

The last Meeting of the Society, before the long vacation, was held at the Society's rooms, Burlington House, Piccadilly, on Friday, June the 11th.

J. R. Hind, Esq., F.R.S., *President*, in the Chair.

Secretaries—W. H. M. Christie, Esq., and J. W. L. Glaisher, Esq.

The minutes of the May meeting were read and confirmed.

Mr. Christie announced that 61 presents had been received since the last meeting of the Society. Amongst them were photographs of the moon and of the Physical Observatory, Oxford, presented by Professor Pritchard.

The following gentlemen were duly elected Fellows of the Society:—

Frederick Hall, Esq., Dancers Hill House, Middlesex.
George Kilgaur, Esq., Mem. Inst. C.E., F.R.G.S., Dutoitspan, Kimberley, Griqualand West, South Africa.
Lieut. D. F. MacCarthy, R.E., Carlisle Fort, Middleton, Co. Cork.
Francis H. S. Orpen, Esq., F.R.G.S., F.R.C.I., Surveyor-General of Griqualand West, South Africa.
Lieut. Sidney Smith, R.N., H.M.S. *Forester*.
Capt. Robert Wright Sterry, 82, Basinghall Street, E.C.
Capt. Peter Thompson, Marine Board, Tower Hill, E.

Professor Pritchard was called upon to read a paper entitled *On the first results of the researches in connection with the lunar physical libration.* After stating that he had three papers to read, and apologising for reading so many at once, he said that it was doubtless known to the Society that he had been, for the last few years, at work with Mr. De la Rue's reflector taking photographs of the moon with the intention of measuring them, and thereby determining the lunar physical libration. They had now obtained some hundreds of photographs, and he had presented to the Society an enlargement made from one of them by an Oxford photographer, which would show the degree of sharpness they had obtained. The photograph from which the enlargement was made was not by any means exceptional. Professor Pritchard also exhibited a photograph of the instrument with which the measurements were made. He stated that it had been presented to the Oxford Observatory by Mr. De la Rue, and was probably the best measuring engine that had ever been constructed. The instrument was far too delicate and valuable to be handed about, and he therefore only exhibited a photograph of it, which would, no doubt, give a very good idea of its construction. Up to the present time they had measured fifty-nine photographs: the method adopted was to select some well defined point—not very far from the centre of mean libration of the lunar disc—and to measure the distance of the point from seven or eight points on the lunar limb. From the fifty-nine photographs which had up to the present time been measured, they found that there was a slight inequality in the lunar rotation amounting to about $1\frac{1}{2}''$ of arc. The difficulty arising from inequalities in the lunar limb was entirely got rid of by the method of measuring the distance of the point of reference from several points on the lunar limb, and if they found that the libration determined from one point of reference almost exactly coincided with the libration determined from measures of an entirely distinct point of reference, there would then be little doubt that the result could be relied upon, and he hoped within a few months to present to the Society the reduction of all the photographs that had been made during the first two years. The remaining photographs must be left for anybody else who had the patience and the skill to undertake the reduction of them. The method they had employed was in the first instance suggested by Bessell, in 1839. In 1846 Wichmann had undertaken such an investigation, and it was satisfactory to find that he had arrived at a result very similar to their own. His measures were made with a heliometer, but there were great advantages in measuring photographs, for while a series of heliometer measures were being taken, half-an-hour or more would elapse, during which time the apparent lunar diameter and the libration were constantly changing; Bouvard and Nicollet had also undertaken similar investigations, and their results all tended to show that there was a small physical libration, which did not differ greatly from the Oxford result.

Mr. Neison: I should not rise to make any remark, if it were not that I think I can make some suggestions which might be of use to Professor Pritchard in his reductions; as he then will be in a position, before completing his labours and bringing them before the Society, to see how far the objections I have to urge have any effect upon his investigations. Now, first of all, I would observe that when Wichmann made his long series of investigations, he found the great objection to the method was in the fact that there were irregularities in the limb at different parts, so that you got at one part of the limb a different diameter to what you found at another. He found when he took his measurement from different points on the same day that they agreed fairly well; but when he had to take the results for different days, it was then that the discrepancy became manifest. The difference amounts to several seconds of arc [Prof. Pritchard: "Oh! Oh!"], and for this reason Wichmann found he could not get reliable results from his measurements. Now these inequalities depend upon the libration of the moon, by which different ranges of mountains are brought in profile on the limb. Supposing there is a high range of mountains, and in front of them there is a valley; at one libration you will get the valley on the limb and a depression, and a few days later there will be the mountain brought on the limb and you will get an elevation. It was this difficulty which prevented Wichmann from obtaining successful results. Now, what I want to point out is that there is a relation between the period by which the libration changes and that of the real libration of the moon, which will produce systematic error. The mere existence of these irregularities on the limb will cause an apparent physical libration in the moon amounting to about one second of arc—just such a quantity as we find has been actually discovered. Now let us go a step farther. Professor Pritchard remarked that he fixed on two points, so that the results from the one would check those from the other. But this will not be the case of the errors of the nature just pointed out, for they will practically affect both in the same way. Professor Pritchard remarked that all results hitherto obtained have been in accordance, and drew the conclusion that this strongly confirmed the reality of the supposed libration. I would point out that this inequality I draw attention to has a period of nearly nine years. It is, in fact, the period of the rotation of the lunar perigee. Every nine years, therefore, the same systematic errors will recur. Now, suppose we take Professor Pritchard's work, and assume that the middle of his observations was about 1878, and take exactly four revolutions backwards, we come to about 1842, which is just the epoch of Wichmann's measures.

Professor Pritchard: Mine were taken in 1876.

Mr. Neison: However, that will not make much difference. (Laughter). Now let us go back a few years. Professor Pritchard refers to results obtained by Bouvard and Nicollet. Then suppose we go back thirty-six years again, and we come to 1806, an important time, when these measurements were begun by Bouvard and Arago; therefore any systematic error which affects one will affect all. But now let us take another one which Professor Pritchard does not allude to, that made by Nicollet himself in 1815 or 1816, just one period from 1806, bringing every one of the four under the same systematic error as I have drawn attention to in the case of Professor Pritchard; so that it is no wonder that they all agree, as if there is any error due to inequalities of the limb it would be systematic, and the same throughout every one of the whole four series of observations. (Applause.)

Mr. Marth: The last year is 1878?

Professor Pritchard: I cannot say. I think 1876.

Mr. Marth: It would be just three revolutions and a half bebetween 1876 and 1845, which was the date of Wichmann's. (Laughter.)

Mr. Neison: 1842-3.[*]

Mr. De la Rue: I am sure, notwithstanding what Mr. Neison has said, he has no intention of decrying all attempts at determining this very interesting problem of the lunar libration, because, unless some attempts are made from time to time with every method at our disposal we shall never be able to arrive at any conclusion respecting it. But it appears to me, and therefore I call attention to this point, that Mr. Neison is in some error as to the method of determining the distance of a lunar crater from the limb of the moon. There are no direct measurements made from

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[*] [Note by Mr. Neison.] Mr. Marth's date is nearly right. At the meeting, speaking roughly from memory, I gave round figures for the dates and periods of the fictitious real libration due to the irregularities in the limb. The correct period gives as the dates within which the inequality would similarly affect all the observations—

1876—1881	...	Professor Pritchard's results, 1876—1877.
1841—1846	...	Wichmann's results, 1844—1846.
1815—1820	...	Nicollet's results, 1819—1820.
1806—1811	...	Arago and Bouvard's results, 1806—1810.

All the observations fall within the same half of the period, and would be similarly affected. Of the existence of such a term there can be no doubt; about its value nothing has yet been made known. There are other difficulties to be met, but these I had not time to point out.—E. N.

any one point of the lunar periphery to a selected spot, and therefore the measurements are not affected by slight inequalities. The centre of the moon is determined as in solar photographs; the centre of the moon in reference to its general contour is determined by the rotation of the micrometer; and gentlemen who take the trouble to observe this photograph of the micrometer will find that there are means of doing it. The centre having been determined by the rotation of the moon against a position line in the microscopic micrometer, the distance is then determined of any one spot from the centre of the *general* circumference of the moon. (Applause.) I was going to ask Professor Pritchard whether, in his determinations, he gets the libration to be expected all in the same direction, or in some cases in an opposite direction? I mention this to ascertain whether the result is at times a negative one: that is to say, I wish to know how many observations confirm his general results, and how many are opposed to them; because if there are a great number of negative quantities, and the final value happens to be only a mean between the positive and negative quantities, the results would not have the same value as if the far greater number were of a positive character. All I wish to add, as an old photographer of the moon, is that the photographs taken at Oxford are of remarkably good definition. (Applause.) Naturally I have taken a great interest in investigating the possibility of making measurements to a very small fraction of an inch; for example, measurements have come out, reliably, certainly, to the five-thousandth of an inch, and I may say, in my own case, to the ten-thousandth part of an inch—that is, the ten-thousandth part of the lunar diameter—and I venture to congratulate Professor Pritchard on the remarkable skill, zeal, industry, and perseverance with which he and his two observers and computers, Mr. Plummer and Mr. Jenkins, have shown in this very laborious undertaking. (Applause.)

Professor Pritchard: I have, within the last few months, been accustomed to hear exaggerations; but I declare that in all my experience I have never heard anything like what has been said this evening. There is Rutherford's moon, and there is mine [pointing to photographs on the wall]. We have not touched them, no human hand has; but that is the work of a human hand [pointing to Mr. Neison's diagram on the black board]. Now, first of all, I will trouble you for a piece of chalk. Mr. Neison is mistaken. This is not the period of the moon's perigee, or anything like it; that is a mistake. Therefore the nine goes nowhere; neither is it seven. The period is extremely difficult to show. Every lunar inequality plays its part in these rotational inequalities, and the man does not live who can say what the period is when they shall all return. Therefore all these had better be done so with [rubbing out Mr. Neison's diagram]. I only want to search after truth. We have done our best. There are not these practical inequalities which Mr. Neison mentions in the moon. I will give you an instance. We have had reason, in searching after truth, to doubt one or two of the photographs, and what was then done was this: they were submitted to Mr. De la Rue's measuring-engine to be done over again, and the result was that the lunar contour is not *that* dreadful thing [pointing to Mr. Neison's diagram]. I cannot draw it even enough, it is so excessively even. (Cries of "No, no!") All we can say is, the problem is one of very great difficulty. It has been now three times attempted. Our results agree more with Wichmann's than Wichmann's do with Nicollet. Of course we cannot doubt that the libration is a physical consequence of gravitation; and it is what we have, on the whole, determined within reasonable limits of accuracy. But perhaps it would be just as well that we all of us should try to help, and if we have ideas, let them be ideas of construction and not of destruction. (Laughter.) If Mr. Neison can point out any better method than we have pursued, I shall be most thankful indeed to avail myself of it, for we have no jealousy. We are simply searchers after truth, and all I can say is that such remarks, founded upon nothings, do not produce either peace or prosperity or lead to the determination of truth.

Mr. Neison: Will Professor Pritchard allow me to say, in reference to the nine he struck out, that he is in error, as this inequality arises from the irregularities of the moon, it must vary with the same period as the revolution of the moon's perigee, which is nine years, as I stated, unless it has changed within the last few days?

Professor Pritchard: That is exactly what I deny.

Mr. Neison: If Professor Pritchard will deny that the apparent libration of the moon, which depends on the equation of the centre of the lunar orbit, depends upon the revolution of the perigee, then I am willing to admit that my attainments are insufficient to enable me to understand his view of the subject. (Laughter.) With regard to inequalities, I was alluding to such inequalities as the range of mountains known as Leibnitz Mountains, which extend along the moon's limb for 700 or 800 miles at a time, and have peaks, which have been repeatedly measured, rising to four miles in height. These are not small irregularities. There are mountains which extend for fifty miles at a time, and have heights ranging from one and a half to four miles, and elevations like these extend at times over arcs amounting to sixty or seventy degrees. It is these which produce apparent inequalities in the diameter of the moon, and have the effect which I drew attention to, and you cannot eliminate them by taking a few measures. Wichmann proves from his measures, made on the same system as Professor Pritchard's, that they give rise to variations of more than one or two seconds of arc from different parts. I do not want to say that Professor Pritchard's results are wrong. I do not know his results. I simply drew his attention to these points, so that he could introduce into his investigation an inequality of unknown value, leaving this period so as to enable him to determine the effect of that inequality. If it amounted to nothing he would be able to ascertain it and show that the value that he found for the real libration of the moon was a thoroughly trustworthy thing, and one obtained without neglecting any precautions whatsoever.

Professor Pritchard: Allow me to say that I have been for four years on this subject, and I do not absolutely deny that the period depends slightly on the equation of the centre. The equation of the centre gives about forty seconds, multiplied by a small quantity, but the thing is so small that Nicollet neglected it. It does not depend on that, but much more upon the annual equation, which is about eight times as large, and there is no such period as nine years connected with the physical libration. I have been four years on the subject, and really, if any light can be thrown, or any improved method given, I shall be most thankful to adopt it. But I see no good in exaggerating all these extreme difficulties. There is really no truth in saying we have discrepancies of three, four, or more seconds. Such quantities are unknown to us. Why, we should be frightened if we found them in any part of our investigations, half a second would tease us. With reference to these exaggerations, it would be well for us not so much to search for objections as to communicate improvements. (Applause.)

Mr. Christie: I feel when I see Professor Pritchard and Mr. Neison take part in this matter that they may explain one or two points I have been a little in doubt about. Professor Pritchard mentions eight or nine measurements, and Mr. Neison talks about six points on the circle. I am not quite clear whether Professor Pritchard measured all these points within an arc of sixty or seventy degrees. Mr. Neison, it appears, considers that his mountains would extend over such an arc. But the question is whether the measurements are made on a small arc or distributed over the whole circumference.

Professor Pritchard: They are distributed all round the illuminated limb of the moon.

Mr. Christie: All round the circumference?

Professor Pritchard: Certainly.

Mr. Neison: All round on one and the same photograph?

Professor Pritchard: Surely the moon has more limbs than one: the first or the second limb, as the case may be.

Mr. Christie: That is the point, whether the measurements are confined to a limited arc or not. I feel a difficulty in understanding how the heights of mountains extending over sixty or seventy degrees can be determined above the general level of the moon. What are the means of ascertaining the true circle from which you are to determine the heights?

Mr. Neison: There is no difficulty in determining the heights. The difference between myself and Professor Pritchard is, probably, due to the fact that Professor Pritchard refers to the inequality in the real libration, and I explicitly stated that I referred to the inequality of the apparent libration, which gave rise to apparent inequalities in the real libration.*

Mr. De la Rue: Reference has been made to Wichmann's measures, and his desire to obtain photographs to measure from. Well, that arose from this circumstance. In 1858 I passed through Königsberg on my way to Russia, and made the acquaintance of Dr. Wichmann. I then had with me some photographs of the moon, one of which I gave him, and he was so struck with the sharpness of them, that he was very anxious to obtain a number. At that time I contemplated settling for a time in Russia, and I told him so, and he had recourse to the Astronomical Society to ascertain if they could furnish him with photographs.

The Society's thanks were then accorded to Professor Pritchard.

Professor Pritchard was then called upon to read a paper on a new form of micrometer which he had devised. It consists of a plate of glass ruled with lines, which is placed in the principal focus of the telescope. The position angle and distance of stars at a considerable distance apart can be measured with the micrometer by means of two eye-pieces, which can be shifted so that each star is in the centre of the field of view of one of the eye-pieces. One of the stars is then brought to coincide with one of the lines ruled upon the glass plate, and the distance of the other star is measured by means of an ordinary micrometer from the nearest ruled line upon the glass plate. By this means the position angles and distances of stars which up to the present time could only have been measured by a heliometer can be determined with great accuracy. He concluded that Mr. Marth would tell him that the instrument had already been discovered in Germany. But at all events it was an independent contrivance of his own, and it was much cheaper to construct than a heliometer. Unfortunately they could not afford them a heliometer at Oxford, and he had therefore devised this instrument, which was a very good makeshift.

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* [Note by Professor Pritchard.] There is a confusion as to the two terms in use for the "moon's librations." This confusion will be removed if we adopt the terms *Perspective Libration* and *Rotational Inequality.* The longitude of the perigee occurs in the expressions for both these librations, but in the rotational inequality it plays but a very secondary part when compared with the effects caused by the annual equation: according to my recent determination the former is about one-eighth only of the latter.

As to the dates of Wichmann's observations and my own, I may say, that so far from the perspective view of the moon having been the same in the two cases, it so happens, accidentally, that the two phases of view were as dissimilar as they possibly could be. Wichmann's mean date is about 1845·5, my mean date is about 1876·6; the difference is 31·1 years, and this period divided by 8·8 (the mean period of the perigee's revolution) gives 3½ periods; thus indicating the greatest possible difference in the two perspective phases. But even irrespective of this incontrovertible fact, I entirely fail to appreciate Mr. Neison's general argument.

Lord Lindsay asked how Professor Pritchard had satisfied himself that there was no distortion due to the object-glass.

Professor Pritchard said: Now, that *is* a practical question, and I shall have great delight in replying to it. (Laughter.) What we have done is this: we had, before we applied this instrument, examined the focal plane of our large refractor, and measured two stars at various distances from the optical centre in a field of twenty minutes, and we find the result is the same; that is, we can depend upon there being no distortion over a field of twenty minutes in diameter.

Mr. De la Rue: I would remind Professor Pritchard that he has overlooked the heliometer at the Radcliffe Observatory.

Professor Pritchard: I was once in charge of the institution. I know that heliometer well, and all I can say is, it has not been effectively used. But if any man can use it, Mr. Stone is the man, and I expect he will have skill and perseverance to turn magnificent works out with it.

Mr. Marth said: I should not rise if it were not in obedience to Professor Pritchard's summons. As far as I remember, the first observer, who used a ruled glass plate, was Tobias Mayer, and indeed, his catalogue of the stars in the Pleiades was made with such a glass scale, which he had constructed with his own hands and poor tools. This was before 1750, perhaps in 1748. At that time there lived at Augsburg a clever mechanic named Brander, who took up the construction properly, and made Brander's glass micrometers, which in the latter half of the last century were in great repute, and much used for different purposes.

A description of some of them was published by Lambert about 1770, in which he proposed for large distances the use of two eye-pieces, and, I think, Brander made several micrometers of that kind. I have never seen one, but I remember having seen it stated that at Augsburg, where Brander lived, the museum contains a collection of some of his instruments, so that to any gentleman present who travels that way, I should suggest to have a look at them, and to let us know what they are. Of course these instruments may not be comparable with the present one in beauty of detail, but I mention them merely as showing the first application of the principle Professor Pritchard has described. (Laughter.)

Mr. Wentworth Erck: I had a micrometer ten years ago for the purpose of measuring the solar spots; mine was a great deal smaller than this one. The plan of mine was, I had a glass divided into small squares instead of single lines, so that there were ten squares to the radius of the sun; the whole of the scale being movable by a micrometer screw. A single eye-piece could be rapidly moved from one part to another. In that way measurements could be made with great ease and accuracy.

Professor Pritchard: The telescope might have moved. The difficulty is to be quite sure that the telescope is not moving while you shift your eye-piece.

Mr. Wentworth Erck: With a good driving clock there would not be much danger of moving in a couple of seconds.

Professor Pritchard then showed a chronograph made from a Morse telegraph instrument which he had contrived. He said: We found it necessary to get a chronograph, and as we found it an expensive matter, we did not think it advisable, under the circumstances, to draw on the pecuniary resources of the University, and necessity being the mother of invention, we invented a chronograph of our own. It was one of Morse's telegraphs that suggested it to me. After looking at it I saw that with little difficulty a Morse's telegraph could be converted into a very effective chronograph. I applied to Mr. Grubb for his assistance, and he carried out my idea, adding some of his own, and the result is a little chronograph which works very well indeed. It gives records to a thirtieth of a second of time, and we are quite sure we are within that interval. It has answered our purpose admirably well, and is a very good substitute for a more costly instrument. The whole cost of it is only £7.

Mr. Common: Will you sell it for £7?

Professor Pritchard: You can get one from Mr. Grubb for £7, and I think it answers all the purposes of a more expensive instrument.

Lord Lindsay: As Mr. Marth answered for Germany, I will answer for Scotland. If Professor Pritchard will pay me a visit I will show him the instrument I used for the transit of Venus, which is a Morse's instrument with two sets of prickers, one to prick the seconds and the other for observations. I used it as a supplementary instrument. I can speak to the fact that the instrument is thoroughly satisfactory with the exception of one point, and that is if the tapes are not read off pretty soon the change in the paper, owing to the hygrometric changes of the air, is such that the marks are gradually effaced, and to use the records is extremely bad for the eyesight. My instrument, however, cost more than Professor Pritchard's.

Mr. Christie: The Morse instrument, or some modification of it, has been used for some time as a chronograph, but it costs more than £7; a Morse instrument costs about £20.

Professor Pritchard: Mr. Grubb will supply you with one like mine for £7.

Dr. Schuster described a paper by Dr. Wiedemann on a method of determining the pressure at the solar surface. He said, that the method depended on the fact that it is only light emanating within a certain time from a given source that will produce interference bands. Professor Wiedemann, therefore, proposes to isolate a part of the spectrum of the light from a comet or a nebula and to count the number of interference bands which are visible. Dr. Schuster thought that the method appeared hopeful, but he suggested that the intensity of successive fringes should be measured photometrically.

Mr. Christie said: The question Professor Wiedemann has proposed is more important to astronomers than at first sight appears. A short time ago our President asked me whether any information could be given as to the pressure at the sun's surface, so as to determine the retardation of a comet in passing through the corona. If the proposed method is successful it will be an important matter, not only as a question of physics, but also in gravitational astronomy, as it would enable us to determine the effect of the solar atmosphere in altering comets' motion.

Mr. Marth said: With regard to a paper forming an addition to his ephemeris for physical observations of Jupiter, that he had received from M. Trouvelot, of Cambridge, Massachusetts, some observations of the red spot on Jupiter made in 1878, and he expressed a hope that observations made in that year might still be forthcoming, to remove any doubt about the early history of the spot, and at any rate that many observers would take part in future observations. The paper, however, referred chiefly to observations connected with the figure of the disc of Jupiter, for the correct reduction of which it supplied the proper data. It would be very desirable to get the existing uncertainty of the true dimensions and ellipticity of Jupiter reduced by first-rate observations.

A paper from Mr. Gill was laid on the table, which will probably be printed in the *Memoirs* of the Society. It gives a complete account of the observations of the opposition of Mars, observed by Mr. Gill, at Ascension, in 1877. The value of the solar parallax which he deduces is $8''\cdot78$ with a probable error of $+''\cdot012$. From the comparison of a single pair of evening and morning observations of the planet, Mr. Gill deduces a value of the solar parallax, with a probable error which he estimates to be less than the probable error of the solar parallax derived from the whole of the transit of Venus observations.

Mr. Chambers asked for information with respect to the announcements of comets to be distributed to fellows of the Society, which were referred to in the *Monthly Notices*, and asked what were to be the conditions on which such notices would be issued to fellows?

The President said: There were many fellows who would not care to receive these notices. The idea had been to send them only to those who applied for them.

The following papers were also announced:

Major-Gen. J. Baillie: *On a simple method of finding a meridian line.*

Rev. J. Pearson: *On changes of sea-level due to atmospheric influences.*

Rev. J. Pearson: *On a partially graphic method of computing solar eclipses and occultations of the moon.*

A. Marth: *Ephemeris for finding the positions of the satellite of Neptune.*

Rev. S. J. Johnson: *Coincidences of sun-spots and auroræ in the olden time.*

A. Wagner: *On the star No. 894 in the first Greenwich seven-year catalogue.*

R. H. M. Bosanquet and A. H. Sayce: *The Babylonian Astronomy, Part III.*

W. Tempel: *Note on the nebula near Merope.* (Extract from a letter to the Foreign Secretary.)

G. L. Lohse: *On the refractive and dispersive powers of various samples of glass.*

T. Bassnett: *On a nebulous solar spot.*

J. C. Adams: *Note on the constant of lunar parallax.*

Sir G. B. Airy: *Addition to a paper entitled, "On the theoretical value of the acceleration of the moon's mean motion in longitude produced by change of eccentricity of the earth's orbit."*

THE VISITATION AT GREENWICH OBSERVATORY.

The annual visitation of the Greenwich Observatory took place on the 5th of June, when the Report of the Astronomer Royal was read and distributed.

This report shows that the admirable condition and effectiveness of the observatory are, if possible, greater than ever in every department of work. "The exceptionably bad weather of the past summer has seriously interfered with the observations, a whole month's observations of the sun being lost in July." In the spectroscopic determination of star-motions 51 stars, of which 21 had not previously been examined, have been observed; and it is expected that in time the motions of about 300 stars may be thus determined. The epoch of minimum of sun spots appears to have occurred about the beginning of 1879. Since last October the outbreak of spots has been very marked. The approximate mean declination of the magnetic needle in 1879 was 18° 40′; mean horizontal force 3·91 in English units; mean dip 67° 37′. The number of chronometers sent for proof is 229, of which 44 are entered for the annual competition trial. Of these 11 are fitted with Sir George Airy's supplementary compensation. The competitive trial of last year shows a slight improvement over 1878, as regards the first 6 chronometers. The first is a very good one. We regret to find that the feeble health of Mr. Lynn compelled him to retire in January, and this has caused a strain in the system of the establishment. The calculations of the Transit of Venus, under the direction of Major Tupman, are still in prosecution. As regards the transit of 1882, the observation of which will be comparatively easy and inexpensive from the rapid and accurate communication of time now given by electric telegraph, the Astronomer Royal repeats the general opinion that it will be useless to repeat photographic observations. The Astronomer Royal's numerical lunar theory is progressing. Much of the expense of this has been his own. In his general remarks Sir G. Airy briefly refers to some important points. These are, the reduction of printing, the increase of annual expense, and the ulterior organisation of the observatory. Besides the great extension of the original work of the establishment in regard to the heavenly bodies, two new departments—Magnetic and Meteorological Observations, and Photographic and Spectroscopic Astronomy have been created; and though at present there appears no necessity for any action, as the three departments are working well and efficiently, circumstances might at a future period interfere materially with the successful continuation, in one place, of this triplicate series of observations,

REVIEWS.

Meteorological Observations made at the Adelaide Observatory during the year 1878, *under the direction of Charles Todd, C.M.G., F.R.A.S.* Observer, Postmaster-General, and Superintendent of Telegraphs. Published by authority of the Government of South Australia, Adelaide, 1879. Pp. xliv. 146.

This elaborate annual volume is as usual most complete in every way. It has many charts and maps, in one of which the rainfall is indicated by circular black spots, the diameters of which are proportional to the amount of rain from 0 to 100 inches. An appendix contains observations on the phenomena of Jupiter's satellites, and notes on the physical appearance of Jupiter, the Transit of Mercury, May 6, and the annular eclipse of the sun, February 1. This work is highly creditable to Mr. Todd and his assistants, and does honour to South Australia.

Results of Astronomical Observations made at the Melbourne Observatory in years 1871-2-3-4-5, *under the direction of Robert L. J. Ellery,* Government Astronomer to the Colony of Victoria, Australia. Melbourne, 1879. Pp. xxi., 207.

These are observations made with the transit circle 5-in. aperture and 72-in. focal length; they are the separate results for R. A. and N.P.D. and catalogues of concluded mean R. A. and mean N. P. D., with the annual precessions. The position of the Melbourne Observatory has been determined with great accuracy. Observations of the tabular errors of the moon's R. A. at the Melbourne Observatory in 1874-75, agreed with the corresponding Greenwich observations, with the difference of only the 100th of a second of time. The personal establishment, besides the chief, consisted, during the period embraced by this volume, of Messrs. E. J. White, C. Moerlin, F. MacGeorge, J. Turner, and J. E. Gilbert, assistant astronomers; Mr. MacGeorge resigned in 1872, and was succeeded by Mr. Turner. The number of stars observed in the abovementioned years respectively, is 266, 253, 377, 414, 349. Many in each year being the same. The determination of stellar co-ordinates with extreme precision is of the last importance in astronomy, and to this the present volume is a very valuable contribution. It is well printed by Mr. John Ferres, Government Printer, and forms vol. v. of the Melbourne Observations.

Cincinnati Observatory. Micrometrical Measurements of 1054 *Double Stars, observed with the* 11*-in. refractor, from January* 1, 1878, *to September* 1, 1879. Under the superintendence of Ormond Stone, A.M., director. Cincinnati. Published by authority of the Board of Directors of the University, 1879. Pp. xxix. 180.

Observations of double stars continue to be the work of this observatory, preliminary in part to the preparation of a general catalogue of those which are situated between the equator and —30° declination. Of the stars contained in this volume 622 are in the S. and 432 in the N. hemisphere. The observers, besides Mr. Stone, were Messrs. H. A. Howe and H. V. Egbert, and Mr. A. S. Flint aided in preparing the work for the press. An achromatic eye-piece magnifying 450 times has usually been employed, but eight non-achromatic with powers from 95 to 1500 have occasionally been used. A full investigation of personal equation in position angle and distance is given, and a list of 74 new pairs (nearly

200 in all have been found) detected during the progress of the observations. Mr. Stone acknowledges his obligations to Mr. Burnham, Mr. Otto Struve, and Baron Dembowski, and to the Manuals of Double Stars by M. Camille Flammarion and Messrs. Crossley, Gledhill, and Wilson, which supply a long-felt want.

Address of Professor Samuel S. Langley, Vice-President, Section A, before the American Association for the Advancement of Science, at the Saratoga Meeting, August, 1879. [From the Proceedings of the American Association for the Advancement of Science, vol. xxviii. Salem, 1879. Pp. 15.]

This is on the subject of Solar Physics, the present study of which may be said to have been inaugurated by the employment of the spectroscope by Kirchoff, in 1860. We should like to have given extracts from this interesting address, which is an able review of the labours of the last twenty years, of how much has been achieved, and how much more remains unknown.

Measures of the Polar and Equatorial diameter of Mars, made at Princeton, New Jersey, U.S. By Professor C. A. Young. [From the American Journal of Science, vol. xix. March, 1880.

On the night of 12th November, 1879, Mars nearly reached its opposition and its node together. Its phase at that time, and for a few days before and after, was extremely small, and an opportunity was presented for determining its ellipticity, such as will not be available again for nearly half a century. For this Professor Young employed the splendid 9½-in. equatorial (by Clark) of the School of Science Observatory, with a filar micrometer. He used a power of 398, and the total number of micrometer readings was 1140. The final result gives the polar compression $\frac{1}{219}$. which is very nearly what Professor Adams arrived at, $\frac{1}{228}$, on the supposition that the law of central density is the same in Mars as in the earth, though Professor Langley remarks, the closeness of the accordance is probably in a considerable degree accidental.

Mémoire à l'appui des remarquables observations de M. Schiaparelli sur la planète Mars. Par M. F. Terby, Docteur en sciences, Membre de la Société Royale Astronomique de Londres, à Louvain. [Présenté à la Classe des Sciences dans la Séance du 6 Décembre, 1879.] Bruxelles: F. Hayez, 1880. Pp. 29.

At the opposition of 1877, M. Schiaparelli, director of the Observatory of Milan, made a minute examination of the continents of Mars, with the excellent equatorial of his observatory, which has extraordinary defining power. He found the continents of Mars divided by a system of canals into a multitude of isolated and detached lands. Dr. Terby has found, amongst a large number of drawings in his possession, some which he thinks tend so much to confirm M. Schiaparelli's map (still incredulously regarded by some) that he has drawn up this paper, which is accompanied by five drawings of Mars by Mr. E. S. Holden, in 1875, made with the great Washington refractor and a power of 400, and one by D. Lohse (1872). These Dr. Terby discusses in detail, and then considers other drawings and observations by different observers at oppositions from 1862 to 1877. He concludes that the map of M. Schiaparelli is the most exact representation of the reality that we know of, and that with it we can explain a crowd of details embodied in the drawings either previous to those of Milan or, at least, entirely independent of them—details which have hitherto remained inexplicable enigmas.

Three approximate Solutions of Kepler's Problem. By H. A. Howe, A.M., Assistant at the Cincinnati Observatory.

The methods here given are intended for use in computing the orbits of minor planets. The author expresses his intention to develop this subject further, and to publish tables for the use of computers.

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1. *Greenwich Astronomical Observations*, 1877. Introduction. pp. 136.
2. *Greenwich Astronomical Results*, 1877. pp. 151.

The first part of the Introduction relates to the Personal Establishment and arrangement of the Observatory. 2. The instruments. 3. Subjects of observation. 4. Explanation of the printed observations. Personal equations are discussed at great length with reference to the chronographic transits. They are referred to Mr. Criswick as the standard. They vary in 16 observers from zero to about two-tenths of a second of time, often much less. All but one are affected by the same sign. As an illustration of the nicety with which sources of error are periodically investigated, the flexure of the transit-circle had been previously found to require a correction of —0″·12. Observations in 1877 give for this —0″·15. Observations of circumpolar stars, for the determination of the latitude of the transit-circle, made in 1877, using Bessel's refractions with a certain diminution, give the colat. 38° 31′ 21″·51, but using these refractions unaltered, the colat. is 38° 31′ 21″·80. The vexed subject of refractions, it will be remembered, was largely discussed at the January meeting of the Royal Astronomical Society. Its importance indeed is very great. The assumed colat. is 38° 31′ 21″·60. For the altazimuth +0″·29 must be added for the difference of latitude of the two instruments. After applying all possible remedies for instrumental and personal defects, and atmospheric influence, it seems there exists yet a fourth source of trouble. It seems certain that the ground carrying both the transit-clock and the altazimuth is subject to frequent and sudden shifts. This instability of ground appears to be connected with sudden changes of atmospheric temperature. This sometimes renders necessary for each day's observations a special determination of the zero of azimuth. The original entries of observations are in all cases preserved, and this applies to every class of observation. Such are a few scraps from this interesting volume, which shows how observational astronomy pursues its grand march at the Royal Observatory, accumulating an immense mass of data of the highest value for the improvement of the science in all its branches.

In the *Results*, &c., we have catalogue of R.A. and N.P.D. for 1877, Jan. 1, of stars (895) observed in 1877, &c. Horizontal and vertical diameters, and R.A. and N.P.D. of sun, moon, and planets, compared with the corresponding results of the *Nautical Almanac*, &c. Observations of γ Draconis with the reflex zenith tube. Micrometer measures and estimations of the positions of the satellites of Mars and Saturn, made with the S. E. equatorial. R.A. and N.P.D. of comet (*b*) 1877 with the same. Phenomena of Jupiter's satellites, and occultations of stars by the moon, &c. We remark that the occultation of Regulus on Feb. 26, was observed with the E. equatorial (6·7-in. aperture), the S.E. equatorial (12·8-in. aperture) and the altazimuth (3¾-in. aperture). The instantaneous disappearance of the star was observed with the S. E. equatorial at 12h. 45m. 43·98s., which is nearly a mean between the E. equat. 43·78s. and the altazimuth 44·28s. mean solar time. Spectroscopic observations, &c., of solar prominences. Examination of the spectrum of the fine sunspot of 1877, Oct. 25—Nov. 7. Measures of displacement of lines in the spectra of stars, &c., and concluded motions in the line of sight. From these we extract the following, omitting fractions of miles, and taking both the measured and the estimated motions: Among stars receding from us are Regulus, at the rate of 24—26 miles per second; α Orionis, 11—22; Sirius, 9—14; Procyon, 20—31; γ Ursæ Majoris, 20—25; δ Ursæ Majoris, 0—4; α Coronæ, 54—68; α Ophiuchi, 61—90. The following are approaching: Arcturus, 16—51; ζ Aquilæ, 60—70; γ Aquilæ, 3—4; α Cygni, 61—70. We have also measures of dark bands in planetary, cometary, and stellar spectra. Measures of positions and areas of spots, and faculæ on the sun's disc on photographs taken with the photo-heliograph, 1874—1877. The mean areas of these, expressed in millionths of the sun's visible hemisphere, is given in a little table for five years, which is as follows:

Year.	Umbra.	Whole spots.	Faculæ.
1873	116	678	2882
1874	83	583	1095*
1875	45	255	475
1876	25	132	226
1877	20	94	168

* This number is too small for the reason assigned in the note.

(The epoch of minimum according to Wolf, of Zurich, was 1878·9, and he finds the sun-spot period in remarkable harmony with that of magnetic variations, both being 11·7 years.)

Anyone is to be congratulated into whose hands the invaluable *Results* may fall. It is condensed from an enormous annual volume which we believe is sent to most public and many private observatories of note throughout the world.

Camille Flammarion, Astronomie Populaire, description générale du Ciel, illustré de 360 figures, planches en chromo-lithographie, cartes célestes, etc. Paris: C. Marpon et E. Flammarion, Editeurs, Galleries de l'Odéon, 1 à 7, et Rue Rotrou, 4. pp. 836. 10 francs. 1880. (Tous droits réservés.)

This large and profusely illustrated volume is dedicated to "the immortal geniuses of Copernicus, Galileo, Kepler, and Newton, and to François Arago, the founder of Popular Astronomy." Notwithstanding the wonderful progress and transformation of the science in the quarter of a century elapsed since Arago died, the author says it would have seemed to him temerity to publish a new "Popular Astronomy," if twenty years of astronomical labours and free discussion had not prepared him for it, and had he not been encouraged by the circulation of above 200,000 copies of his various works, and the increasing sympathy of such a multitude of readers. Notwithstanding the fine publications of MM. Guillemin, Delaunay, Faye, Dubois, and Liais (only to speak of French authors), he ventures, nevertheless, to offer this work as entirely new in its character and method of exhibition, whose highest aim is to be altogether popular without ceasing to be scrupulously exact, and worthy of the incomparable science to which it is devoted. Beginning with the earth, there follow in succession, moon, sun, planetary worlds, comets and shooting stars, the stars, and the sidereal universe. Many of our readers are no doubt familiar with the animated and picturesque style of this writer, who has done so much to diffuse a knowledge of astronomy in his native country, though we are not aware that any one of his numerous works has appeared in an English dress. A translator would need, in the present case, to use the pruning-knife to suit the different taste of our countrymen; and he would probably dispense with some of those illustrations which are merely fanciful, and perhaps with some of the digressions, although they are always instructive and entertaining. For instance, speaking of the harmony of the worlds, M. Flammarion goes on eloquently to show that there is harmony everywhere: harmony in the undulations on a disc of mercury, of which a pretty illustration is given; in the undulations of sound (Chladni); in musical vibrations (Lissajous); in the crystals of snow; of all which we have the familiar illustrations. Observing that "all is relative," and that the possible dwellers on the minor planets must be organised suitably to the smallness of their abodes, we have in a note a print and description of the minute carriage drawn by two fleas, which was exhibited some years ago in Paris. "There may be worlds where fleas are elephants."

With some of the author's reflections we find ourselves entirely at variance, as, for example, about a table of the transits of Venus he writes, "Voici les dates de ces passages, depuis l'invention des lunettes jusqu'au XXXme siècle de notre ère, ou du moins jusqu'à cette époque, car il est douteux que l'ère chrétienne, qui est déjà vieille de dix-neuf siècles, dure jusque-là. Les opinions humaines changent si vite!" p. 293, and other passages are not in good taste, as on pages 312, 603. From some speculations also, as, *e.g.*, about the origin of life, p. 650, we altogether dissent. We are surprised that M. Flammarion does not attach credit to the discovery of the intra-mercurial planets by MM. Watson and Swift. On the doubtful question of a satellite of Venus, he writes, "Has it fallen on the planet? that is the last possible hypothesis. Have all the observers been wrong? Assuredly not. How then are we to explain those apparitions and the disappearance? It is probable that Venus happened at those epochs to pass before one of the numerous little planets situated between Mars and Jupiter." Many of the illustrations are new and interesting, *e.g.*, the conspectus of the distribution of the minor planets, by General Parmentier. Some are reproductions of curious old prints. The attempt to reproduce on a small scale Mr. Proctor's chart of 324198 stars, fig. 348, is a failure, for it is little more than a black surface. Amusing anecdotes are frequently narrated. A supplement of the work is promised, which will contain tables, charts, and other matter for which room could not be found in this volume, the price of which, we may observe, is very moderate, considering the immense mass of information agreeably conveyed to the mind, and also pleasantly to the eye, in respect of type and paper. We trust the author will be repaid for the great trouble he must have taken, by an extensive circulation in France, and also the appreciation of many in this country and elsewhere.

It may be good for the future improvement of this fine work, to indicate some errata and mistakes that we have noticed:

Page 262. The solar eclipse of 2090 A.D. will not be total at London, though Brighton may be just within the north limit of totality—see "Eclipses, Past and Future," &c., by the Rev. S. J. Johnson. James Parker and Co., pp. 92—94. Mr. Johnson says that a long search extending over 500 years hence has not revealed one solar eclipse total at London.

Page 270. In the first table and last column, Mars should be 1 an 322 jours, Jupiter 11—215 jours, Uranus 84—6 jours.

Page 309. The earth receives in a year from the sun as much heat as would boil an ocean 66 *feet* deep from freezing point. The mistake of miles for feet has been followed by the author, as in various preceding works in this country. (See Becket, "Astronomy without Mathematics," page 103, 5th edition.)

Page 322. It is not to the solar spots that Virgil alludes (Georg. I.), nor, as far as we know, are they referred to by any classic author.

Page 423. The name Heralides should be Heracleides.

Page 439. The anecdote of Copernicus about the phases of the inferior planets was believed to be apocryphal by Professor De Morgan. (Chambers, "Descriptive Astronomy," page 71.)

Page 487. The moons of Mars are said to produce tides on the planet. We fancy this must be an oversight, as they are so small.

Page 332. Fig. 138 should be 137.

Page 353. Line 17 from bottom, 20 jours should be 270.

Page 598. The name Sazoncene should be Sozomène.

Page 624. Pastorff's observation of a comet transiting the sun is very doubtful. See Hind's paper read before the Royal Astronomical Society, May, 1876 (*Astronomical Register*, vol. xiv., page 129).

Page 667. Dun-le-Poëllier must be a corruption of an Indian name, but we cannot at present say of what.

We have omitted to notice a chromo-lithograph which attempts to give an idea of the effect of the play of lights occasioned by four coloured suns and two moons, shining upon rocks and water on a planet belonging to some quadruple stellar system, "whose worlds never know night, where astronomy is impossible, since the heavens are never seen spangled with stars, and whose inhabitants are strangers to sleep." The map of the Pleiades by Wolf, of the Paris observatory, is probably the fullest and best in existence. Perhaps M. Flammarion will give us a table of the positions and magnitudes of the stars laid down on it in the Supplement.

Lectures on Practical Astronomy and Astronomical Instruments. By the Rev. James Challis, M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge, and Fellow of Trinity College. Cambridge: Deighton, Bell & Co.; London: George Bell & Sons. 1879. pp. 400. 10s.

This work is founded upon a syllabus of lectures delivered at the University. Twenty-five years' experience in continuous astronomical observations and calculations has enabled Prof. Challis to produce a work which exhibits with a great degree of completeness the actual state of observational astronomy, especially as relates to the methods in use at observatories with fixed instruments. With these,—the transit instrument, mural circle, transit circle, equatorial, fixed zenith sector, and transit in the prime vertical—the volume is principally occupied, though a portion is also devoted to transportable instruments and miscellaneous astronomical information, which the failing health, we regret to find, of the author, did not allow of his treating with the same fulness as the preceding subjects. We have read the whole with some care, and think that whilst its great value as a text book for students of the science at the universities, and those who are called to serve in public and well appointed private observatories, cannot fail to be appreciated, many others also will be gratified and instructed by its study, and even without access to the grand instruments in question, be able to gain a fair insight into their characters, adjustments, the methods of observing, and reducing the observations. Clear plates of the Cambridge transit instrument, the 8-feet mural circle, the transit circle, and of the Northumberland equatorial, &c., with reference letters, illustrate the text, and although without an index there is a copious table of contents. The theory and use of these instruments are clearly and fully explained, and their treatment throughout evinces the power and originality of a master.

Professor Challis says in the preface: "It seems to me that the tendency of our mathematical instruction and examinations has been of late years to promote the acquisition of a knowledge of formal relations of symbols and the power of readily producing them, apart from a distinct exercise of the reasoning faculty. Now, as far as regards practical astronomy and astronomical instruments, it may be asserted that it would be impossible for the student to make himself acquainted with this department of science without understanding the *reasons* of the mutual relations of the different parts of the instruments, and the processes of observation by which the intended purposes are effected. He must be able to see *how* the many ingenious mechanical contrivances which the wants of astronomy have called forth, contribute to facility and precision in making and recording observations, and although this accomplishment may not demand a very high order of intelligence, it is still a mental exercise of much educational value, inasmuch as it is altogether unlike any process of reasoning by abstract symbols, and may serve as a corrective of the effect of too exclusive an attention to reasoning of that kind. The amount of mathematical knowledge which a complete understanding of practical astronomy requires is not more than what every candidate for mathematical honours is expected to possess, and accordingly the subject may be regarded as being within the reach of candidates of all degrees of proficiency. For these reasons I think it is much to be desired that lectures on practical astronomy should always form a part of university teaching, and that the subject should at the same time receive due recognition in the Senate House Examination."

The Cambridge Observatory was founded in 1822—24. The first director, Professor Woodhouse, was succeeded in 1828, by the present Astronomer-Royal, who was succeeded, in 1836, by Professor Challis. The present director, Professor Adams, was appointed in 1861. Of Professor Woodhouse it has been remarked that in his Treatise on Astronomy he makes the reader feel that he is in the very observatory itself, and the very observations which are cited are made to smell of the instruments which gave them. And we think the work before us is deserving of similar high praise. The expense of its publication has been defrayed by the liberality of the Syndics of the University Press. To illustrate the degree of accuracy obtainable by the combination of observational skill with a first-class instrument, we find that the co-latitude of the Cambridge observatory was determined by Professor Airy in 1833, by 917 observations of circumpolar stars, to be 37° 47′ 8″·43. But combining with this 1190 observations by the author in 1836—38, it comes out 37° 47′ 8″·37 (pp. 210—12). A difference of latitude equal to 0″·1 corresponds to only ten feet on the earth's surface. Besides a few indicated, we noticed two trivial errata; p. 245, 283° should be 280°, and p. 249, 84m. should be 14m.

We may add that notwithstanding the latter portion of these Lectures has not been treated with the almost exhaustive fulness which characterises the former and principal part, it is not greatly wanting in detail, and a fair amount of space is devoted to the theodolite and the sextant.

To conclude; this book explains to us in what manner have been laid the foundations of the present proud fabric of exact astronomical science. It makes us acquainted with the marvellous instruments, triumphs of optical and mechanical skill, to which (after being tested in all points with searching severity) in the hands of trained observers, the sailor and the traveller, and everyone who makes extra-meridional observations, are so largely indebted for the value of their work. And apart from such utility, the precision with which angles and time are now measured at observatories like that of Cambridge will one day, we may hope, lead to the settlement of problems not yet thoroughly solved, and questions still more or less open; and any institution that contributes to the enlargement of our comprehension and the accuracy of our knowledge of the heavenly bodies is well worth all the money laid out upon it, and all the continuous toil expended by those who are devoted to its service.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JULY, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Mount Taurus.—An extensive mountain region between the *Mare Crisium* and the *Mare Serenitatis*, in the north-west quadrant of the moon. Its north-west boundary is marked by the chain of craters stretching from the northern boundary of the *Mare Crisium* to the twin formations, *Atlas* and *Hercules*. Of the chain of craters *Cleomedes* is the largest; it is followed by *Burckhardt*, *Geminus*, *Berzelius*, *Franklin*, *Cepheus*, and *Ousted*. The main bulk of *Mount Taurus* consists of the irregularly elevated mass of high land south-east of *Geminus* and *Berzelius*. On the south-west of the high land is a bright plain, extending from *Burckhardt to Macrobius*. This bright plain has on its borders the following craters: *Cleomedes*, *Macrobius*, *Miraldi*, and *Lithrow*. *Cleomedes* is a large walled plain, 75½ miles in diameter, with broad walls rising on the west to 8735 feet, and on the east to 9706 feet above the interior. On the west and east angles of the south wall are two craters, at each of which occurs a pass, the eastern communicating by a valley with the *Mare Crisium*. From the western a line of eruption extends obliquely across *Cleomedes* to the north-east angle, where the steep, precipitous crater, *Tralles* is situated. The depth of this crater is 13,697 feet beneath its east wall, and it is said to retain the shadow [of the west wall] until three days after full. It is very important to ascertain the correctness or otherwise of this statement, which may be easily accomplished by recording the extent of the interior shadow, in tenths of the diameter of *Tralles*, whenever it is observed. Schröter drew, north-east of *Tralles*, in 1788, Dec. 2nd, a deep depression with a crater on its east border; indications of this depression were seen by Birt in 1859, April 7th and 18th, and June 19th; the line of eruption, as seen by Schröter in 1794, May 3rd and 4th, consisted of a crater on the south-west part of the floor, marked by him γ; it was seen by Birt in 1859, and by Durrad in 1873; a crater on the south part of floor, between γ and the central mountain seen by Durrad, 1873, Oct. 7th; the central mountain itself, consisting of three peaks—Schröter missed these peaks, but they were well seen by Durrad, 1873, Oct. 7; a ring, with a crater on its west portion, Schröter had previously seen, in 1788, Dec. 2, a mountain in this neighbourhood, where Neison gives three compactly placed craters; he also mentions that Smitherson observed, on the western part of the floor of *Cleomedes*, nine lozenge-shaped markings that have not been seen since.

Spots on Plato.—Mr. Stanley Williams, of Brighton, observed, Jan. 27th, 10 50, 1880, on the floor of Plato, with an aperture of 2¾ inches, nine spots, which he identified as Nos. 1, 3, 4, 5, 13, 14, 16 and 17, also one very near the north-east border of Plato, which had not been observed before.

2, Percy Terrace, Cedars Road,
Water Lane, Stratford: May 19, 1880.

ASTRONOMICAL OCCURRENCES FOR JULY, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Vega.
Thur	1	12 48	Occultation reappearance of B. A. C. 782 (6½) Sidereal Time at Mean Noon 6h. 39m. 39·67s.	1st Ec. D.	12 41 42	11 51·3
Fri	2		Sun's Meridian Passage 3m. 49·56s. after Mean Noon	1st Sh. E. 1st Tr. E.	12 16 13 38	11 47·4
Sat	3					11 43·4
Sun	4			3rd Sh. I.	13 15	11 39·5
Mon	5			2nd Ec. D.	11 52 32	11 35·6
Tues	6	22	Conjunction of Moon and Venus 1° 18′ N.			11 31·6
Wed	7	1 21	● New Moon Eclipse of the Sun invisible at Greenwich	2nd Tr. E.	11 51	11 27·7
Thur	8			1st Ec. D.	14 35 39	11 23·8
Fri	9	12 14	Conjunction of Moon and Mercury 2° 48′ N. Jupiter at quadrature with the Sun	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	11 55 13 18 14 10 15 32	11 19·8
Sat	10	8	Conjunction of Moon and Mars 5° 28′ N.	1st Oc. R.	12 40	11 15·9
Sun	11					11 11·9
Mon	12			2nd Ec. D.	14 28 23	11 8·0
Tues	13	12	Superior conjunction of Venus and Sun Saturn's Ring: Major axis=39″·86 Minor axis=10″·66			11 4·1
Wed	14	18 15	☽ Moon's First Quarter	2nd Sh. E. 2nd Tr. I. 2nd Tr. E.	11 20 11 26 14 6	11 0·2
Thur	15		Illuminated portion of disc of Venus=1·000 Illuminated portion of disc of Mars=0·971	3rd Oc. D. 3rd Oc. R.	12 54 15 18	10 56·2
Fri	16		Sidereal Time at Mean Noon 7h. 38m. 48·03s.	1st Sh. I. 1st Tr. I. 1st Sh. E.	13 49 15 12 16 4	10 52·3

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Vega.
Sat	17	8 1 9 15 10 39 11 21	Occultation of B.A.C. 5253 (6) Reappearance of ditto Occultation of B.A.C. 5286 (6½) Reappearance of ditto	**1st Ec. D.** **1st Oc. R.**	10 58 5 14 34	10 48·8
Sun	18		Sun's Meridian Passage 5m. 59·47s. after Mean Noon	**1st Tr. E.**	11 54	10 44·4
Mon	19					10 40·5
Tues	20	15 39 13	Near approach of B.A.C. 6607 (6) Saturn at quadrature with the Sun			10 36·6
Wed	21	9 2	○ Full Moon	**2nd Sh. I.** **2nd Sh. E.** **2nd Tr. I.**	11 12 13 56 13 59	10 32·6
Thur	22			**3rd Ec. D.** **3rd Ec. R.**	11 8 56 13 47 14	10 28·7
Fri	23			**2nd Oc. R.** **1st Sh. I.**	11 45 15 41	10 24·8
Sat	24			**1st Ec. D.**	12 52 7	10 20·9
Sun	25			**1st Tr. I.** **1st Sh. E.** **1st Tr. E.**	11 33 12 26 13 46	10 16·9
Mon	26			**1st Oc. R.**	10 54	10 13·0
Tues	27	15 8 16 24 8	Occultation of 101 Piscium (6) Reappearance of ditto Conjunction of Moon and Jupiter 6° 58′ S.			10 9·1
Wed	28	11 40 3	☾ Moon's Last Quarter Conjunction of Moon and Saturn	2nd Sh. I.	13 48	10 5·1
Thur	29	14 53 15 4	Occultation of ζ Arietis (4½) Reappearance of ditto	**3rd Ec. D.**	15 10 21	10 1·2
Fri	30			**2nd Oc. R.**	14 14	9 57·3
Sat	31			**1st Ec. D.**	14 46 13	9 53·3
AUG. *Sun*	1			**1st Sh. I.** **1st Tr. I.** **1st Sh. E.** **1st Tr. E.**	12 4 13 24 14 19 15 37	9 49·4

THE PLANETS FOR JULY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	8 31 15	N.19 49	7″·0	1 51·3
	9th	9 5 5	N.16 7½	8″·2	1 53·6
	17th	9 23 28	N.12 53	9″·4	1 40·4
	25th	9 23 28	N.11 3½	10″·6	1 9·0
Venus ...	1st	6 33 41	N.23 39	9″·6	23 50·1
	9th	7 16 30	N.23 0½	9″·6	* *
	17th	7 53 29	N.21 51	9″·6	0 10·7
	25th	8 34 53	N.19 53½	9″·6	0 20·5
Jupiter ...	1st	1 6 1	N. 5 37	37″·2	18 23·3
	9th	1 9 5	N. 5 53½	38″·2	17 54·9
	17th	1 11 30	N. 6 6	39″·2	17 25·9
	25th	1 13 13	N. 6 14	40″·2	16 56·2
Saturn ...	1st	1 46 18	N. 8 23½	15″·6	19 3·5
	9th	1 48 5	N. 8 31½	15″·8	18 33·8
	17th	1 49 30	N. 8 37	16″·2	18 3·8
	25th	1 50 33	N. 8 40½	16″·4	17 33·4

Mercury sets about an hour and a half after the sun at the beginning of the month, the interval decreasing.

Venus is too close to the sun to be well observed.

Jupiter rises about midnight on the 1st day of the month, and then earlier each night.

Saturn rises a little before midnight at the beginning of the month, after which he rises earlier each night.

TO CORRESPONDENTS.

All communications should be addressed to the Editor.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

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The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

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ASTRONOMY IN ITALY.

L'Astronomie pratique et les Observatoires en Europe et en Amérique depuis le milieu du XVII^e^ siècle jusqu'à nos jours. Par C. André, G. Rayet, et A. Angot. Cinquième partie.

Observatoires d'Italie. Par G. Rayet, Professeur d'Astronomie physique à la Faculté des Sciences de Bordeaux. Paris, Gauthier Villars, 1878.

After the death of Galileo, in 1642, there is little to note about observational astronomy in Italy till the labours of J. D. Cassini (born at Nice), who, however, was called to France by Louis XIV. in 1669, after which interest in the science slumbered till its revival in the beginning of the eighteenth century. Hevelius and Huyghens in Holland, Cassini, Picard, and Roemer in France, Flamsteed and Newton in England—especially the discoveries of the last mentioned—gave rise to this awakening, and all over Italy the universities, towns, religious corporations, sovereigns, great and small, created observatories and chairs of astronomy. At Bologna the university and the Count Marsigli founded (1723) the observatory over which Manfredi and Zanotti presided. At Milan, the Jesuits created another (1760,) where Boscovich, Reggio, Cesaris, Oriani, and Carlini successively observed, and of which M. Schiaparelli is the present director. At Padua, the Republic of Venice founded, at great expense (1767), an astronomical establishment which has counted amongst its astronomers Toaldo, Chiminello, and Santini. At Florence, Leopold of Austria caused to be constructed, and installed in a corner of the Pitti Palace, instruments with which Amici and Donati have successively worked. At Rome, the Company of Jesus transformed (1787) into an observatory one of the corners of the Collegio

Romano, and G. Calandrelli commenced labours which were afterwards continued by Ricchebach, Conti, de Vico, and the regretted P. Secchi. Lastly, at Palermo, Ferdinand IV. founded (1789) an observatory which Piazzi rendered famous by the discovery of Ceres, and by his catalogue of stars, and which was afterwards presided over by N. Cacciatore, G. Cacciatore, and the well-known M. Tacchini.

Even the campaigns of Bonaparte could not arrest this noble rivalry of cities and of corporations, and the continual improvement of apparatus by which one observatory was ever surpassing another excited the mutual emulation of the states, whilst the applications of astronomy to geodesy, cartography, and navigation, maintained the public interest in the science. Thus were created in succession the observatories of Naples (1812), where M. A. de Gasparis discovered nine minor planets; of Turin (1820), of Modena (1819), and of the Capitol (1825), into which M. Respighi is now infusing new life. The emulation of all these establishments, and the varieties of method by which they were conducted, led to great activity and the rapid progress of all branches of astronomy; a period of prosperity that lasted till about the end of the first third of the century. It was then that the large and costly instruments which the great national observatories with large resources began to have constructed, and their superior collocation, left far behind the limited means of the little states into which Italy was then divided. Universities that with difficulty vegetated in the cities were quite unable to replace with better their ancient telescopes, defective apparatus and bad localisation. The heads of the various Italian observatories were sensible of their inability, with the instruments at their disposal, and their meagre resources, to contend with their more favoured brethren of Greenwich, Paris, Pulkowa, Berlin, and Washington, and for the most part yielding to discouragement, sometimes ended by renouncing labours destined to remain sterile, and allowed their establishments to perish. A united Italy has happily put an end to this state of torpor, and given to astronomers the hope of better days: both the government and men of science vie with one another to restore to the native land of Galileo the high rank which it justly claims.

In August, 1875, an astronomical congress assembled at Palermo, under the auspices of government, voted a plan of reform of which these are the principal points. None of the numerous existing observatories are to be suppressed, but they are to be divided into three classes. 1. Those of Naples, Florence, Palermo, and Milan to be considered as establishments of the first order, and the available resources of the government are to be concentrated upon them. 2. Those of Parma, Bologna, and Modena are attached to the universities of those three cities, and are to confine their labours to meteorology and physics. 3. Those of the Roman College, of the Capitol, of Turin, and of Padua are declared University Observatories, and devoted above all to the instruction of young astronomers. M. Rayet observes that the multiplicity of the centres of observations—a requisite condition for the independence of astronomers, a security for freedom in the methods employed, a fruitful source of emulation, and an active stimulant of the spirit of invention,—is useful, indeed necessary, for the development of the science.

We now proceed, still under the guidance of M. Rayet, to notice briefly the different observatories of Italy. And 1, that of TURIN, which grew out of the geodesical labours of M. Beccaria in the kingdom of Sardinia (1759), who chose for his fixed instruments the summit of a tower surmounting his own house, from which they were afterwards transferred to the palace of the Academy. The construction, begun in 1790, was still unfinished in 1811 (15th March), when Plana succeeded the Abbé Valperga, of Caluso, as Professor of Astronomy to the Faculty of Sciences at Turin, and was afterwards charged with the direction of the observatory (March 5th, 1813). That establishment possessed nothing but an astronomical clock and a repeating circle of 18 in. diameter. The king, Victor Emmanuel the First, had a taste for astronomical studies, and patronised those who cultivated them. With a sum of 12,000 francs accorded by the government, Plana had a meridian circle constructed by Reichenbach and Fraunhofer of the same size as that of Bessel at Königsberg; a repeating circle of 18 in., a small equatorial, and a telescope 6·6 in. aperture. The observatory of 1791 was quite inadequate to receive these instruments, and it was concluded to build a new one on the summit of one of the four massive towers which formed the corners of the Palace Madame. The platform of the tower is 120 feet above the level of the ground. The work was begun about 1820, the expense being defrayed from the private purse of the king, and was finished in 1822, when the instruments had already arrived some months. Meanwhile Plana had taken a large share in the important geodesical operation of uniting the triangulations of Piedmont and Upper Italy to the triangulation of France. The former had been carried on from 1750 to 1811, by Boscovitch, Beccaria, Oriani, and Puissant, and this junction was executed by Plana and Carlini. After this, Plana resumed his astronomical observations which he carried on till his death. The first series was published at Turin in 1828, and is preceded by an important memoir on astronomical refractions. He had also commenced a work which was to occupy him during long years, and withdraw him increasingly from practical astronomy. This was researches on the moon's motion, in which Carlini was associated, and their joint memoir obtained the Lalande prize in 1820, though not published till 1832, after receiving a large number of additions from Plana alone. It forms three large quarto volumes. Prof. Grant (*History of Physical Astronomy*) remarks: "The entire researches of this distinguished geometer exhibit the most commanding mastery of his subject. It is impossible to repress a feeling of sympathy for the author, when he informs us that throughout all the immense calculations of this work, he had not had the benefit of a single assistant." The death of this illustrious astronomer 20th January, 1864, led to considerable changes in the organisation and the *personnel* of the observatory, which, by a royal decree of 1865, was attached to the university, and its director made the professor of astronomy. This post is now filled by M. Dorna, born at Asti in 1825. He had been entrusted by Plana several times with the execution of laborious calculations. When he succeeded Plana, in 1865, he found the observatory had been much neglected for some years. Progress, however, has been made for its recovery. Its publications are become regular, and the *Bollettino Meteorologico* of the university contains every year a certain number of special astronomical observations, especially of the swarms of the Leonids and Perseids, with charts of the trajectories of the principal meteors. M. Dorna has prepared special celestial charts for the marking down of shooting stars. This atlas, published in 1870-71, contains twelve maps on the stereographic projection on the horizon of 45°. It contains stars from the first to the fourth magn., and a certain number of the fifth, so as not to leave any too great void in the heavens. It is employed with great advantage in the Italian stations under the control of M. Schiaparelli. A signal ball, constructed in 1865, gives the time to the city. M. Rayet says the time is not distant when the astronomers of Turin will be enabled to make special researches. He saw at Turin, in 1875, a large cupola destined for an equatorial of 4·6-inch aperture, with which M. Dorna observed the transit of Venus in Bengal. Spectroscopes were already obtained, and M. Charrier, assistant at the observatory, proposed to use the new equatorial for spectrum analysis on the plan of the Society of Italian Spectroscopists. M. Dorna's great ambition, however, is to replace this equatorial by a telescope of at least 10-inch aperture suitable for all the researches of physical astronomy.

MILAN.—The observatory of the College of Brera had its rise in 1760. The R.R.P.P. Pascal Bovio and Dominic Gerra desired to verify for themselves what they had been taught respecting the movements of the heavenly bodies. They had a telescope of forty feet focus, an armillary sphere of iron, and a pendulum clock. With the aid of the *Uranographie* of Bayer, they familiarised themselves with the principal stars, when the apparition of a comet at once stimulated their zeal, and made them sensible of the poverty of their appliances. The Rector, R. P. Pallavicini, a learned man and a lover of science, aided them to the extent of his power; but, as they could not afford the purchase of a large quadrant, they had one constructed, under their oversight, by a clever locksmith of Milan, which, however, in spite of all pains, proved a failure. About 1762 Pallavicini, associated with his two students of astronomy, the R.P. Lagrange, from the Observatory of Marseilles; and in July, 1763, the R.P. Boscovitch, Professor of Mathematics at the University of Pavia, in conjunction with Pallavicini, fixed upon a better site in the College of Brera for the observatory, the plans of which he made, and superintended the construction, moreover generously aiding with his own contributions of money. In 1770 Boscovitch himself had charge of the observatory, where he assiduously observed till 1772, in spite of great want of instruments. Subsequently the Jesuit Fathers, who had expended more than £2,400 on the observatory, thought they could dispose of it at their pleasure, and restored the direction to Lagrange, who although advanced in years, did all in his power to promote its efficiency, and it became well furnished with instruments. To turn these to account, Reggio, de Cesaris, and later, Oriani (1776) were attached as astronomers or students—all three destined to give celebrity to the observatory and the ephemerides of Milan.

Francis Reggio, born at Genoa in 1743, studied philosophy under D. Gerra (before mentioned), at the College of Brera, who encouraged his passion for astronomy, and he became one of the most ardent disciples of Lagrange and Boscovitch, and a proficient in practical and theoretical astronomy. On the retirement of the former, in 1777, he succeeded to the direction of the observatory. With other observations, Reggio occupied himself especially in perfecting the theory of the sun. Oriani undertook researches into the movement of the planets, and in particular of Uranus, for which he computed tables. Applying to Uranus Laplace's method of calculating the perturbations of Saturn by Jupiter, he determined the influence of Saturn on Uranus, and reconciled the modern observations of that planet with those of Flamsteed in 1690 and of Mayer in 1756. In 1791 a mural quadrant, by Ramsden, was added to the observatory, and two years later a Newtonian telescope by Herschel, 6·6-inch aperture and 7 feet focal length. The last work of Reggio, who died at Milan in 1804, was a catalogue of stars of the first four magnitudes visible at Milan, and of Zodiacal stars to the seventh magnitude. He was succeeded by his pupil Oriani, who had been entrusted with the greater part of the calculations for the *Effemeridi* and whose memoir on Uranus has already been referred to. In 1786 he made the tour of the various observatories of Europe, and made the acquaintance of Maskelyne, Herschel, Ramsden, Dollond, Laplace, Lalande, and Méchain, and on his return to Milan resumed his investigations of Uranus. It was through Oriani that Cagnoli, whose private observatory at Verona was destroyed at the siege of that city by the French, was brought to Milan. Originally engaged in the diplomatic service, when at Paris in 1776, Lalande showed him Saturn's ring. From that day his vocation was decided, and he devoted himself entirely to astronomy. He purchased instruments, and studied mathematical astronomy with Lalande. In 1785 he presented to the Académie des Sciences a memoir on the determination of the equator of a planet, and published his celebrated *Trigonometry*. He had also commenced a series of observations of stars to the seventh magnitude; but his strong desire to revisit Italy took him to Verona at the end of 1784. There he erected a new observatory for the instruments he had brought from Paris. The siege of Verona failed to distract Cagnoli from his occupations till a bomb fell upon his observatory. At the request of Lalande, his instruments were purchased for the Milan Observatory, and it was there that he completed his catalogue of 500 stars. He died August 6th, 1816. To return to Oriani: At the death of Reggio he was appointed director of the Observatory of Brera, and continued his astronomical and mathematical labours. His observations and calculations of the new minor planets and of the sun appeared annually in the ephemerides of Milan. The observations for the determination of the obliquity of the Ecliptic were made with a repeating-circle 3 feet in diameter, made by Reichenbach and Utzschneider, and originally intended for the observatory of Zach, at Eisenberg. Observations of thirty-two circumpolar stars, made with this instrument, led Oriani and his pupils, Carlini and Brioschi, to conclude for the latitude of Brera 45° 28′ 0″.70, with scarcely an uncertainty of 0″.5. In the reduction of the solar observations the chief difficulty was refraction; and Oriani was obliged to discard the tables of Mayer, Bessel, and Laplace, and made use of those of Carlini (1808), specially prepared for the climate of Milan. The observations of summer and winter became thus sufficiently in accord. This subject was taken up again by Oriani in later years. In 1827 were published the geodesical operations made in 1809-10 for the verification of the arc of meridian between Rome and Rimini, and in 1821, for the arc of meridian between Milan and Genoa, and the triangulation of Lombardy. After 1830, age and infirmities withdrew Oriani from astronomy, and he died at Brera, November 12th, 1832, aged eighty. His merits were appreciated over all Europe, and he received considerable honours. Napoleon I. even wanted to make him a bishop. This he steadfastly declined, but accepted a pension derived from the revenues of the intended see. By reason of his simple habits, which the possession of ample means had never altered, he left a considerable fortune, most of which he bequeathed to Plana, and to the endowment of the Observatory of Brera, of which he had been the pride.

Angelo Cesaris, his warm friend, had died some months previous. He was born at Pusterlengo, in the territory of Lodi, and studied under Lagrange and Boscovitch, to whom he owed his taste for astronomy, and who introduced him to the observatory in 1773, at its reformation. Soon after, he commenced by himself the publication of the Milan Ephemerides, which he continued, almost without assistance, for twenty-eight years. [This publication, carried on without interruption from 1775 to 1875, now consists of 100 volumes. After Cesaris, it was under the care of Carlini and Schiaparelli.] This wearisome labour did not prevent his observing the sun and planets with the mural quadrant of Ramsden, of the graduation of which he had made a special study. To Cesaris is due the first investigation of the effects upon instruments of the oscillatory motion of edifices occasioned by the unequal action of the sun's rays. He likewise constructed a meridian line, in the Duomo of Milan, of unrivalled accuracy. He died in 1832, and was succeeded by Carlini, who had been long familiarised, at the College of Brera, with all the branches of astronomy. Carlini published new tables of the sun in 1811, of which an improved edition appeared in 1832. In 1813 he joined Plana in the elaboration of a complete lunar theory, which has already been mentioned. After laborious days spent in calculations, he ascended every night to the top of the observatory, to determine the positions of the moon and planets for the correction of the elements of their theories. In the earlier part of his life he took considerable part in the geodesic labours in Upper Italy. Up to eighty-nine years Carlini retained an almost youthful activity, and continued to observe every night. At his death, in August, 1862, the succession devolved on M. G. Schiaparelli, who had been second astronomer since 1860.

M. Schiaparelli was born at Savigliano in upper Piedmont, in March, 1835. Originally by profession an architectural engineer, for which he had not much taste, he obtained from the Government the means of going to Berlin to study astronomy under Encke. For three years he was one of the most assiduous scholars of that celebrated astronomer. In 1859 he went to Pulkova for a year, whence he returned to Brera, to take up the duties of the post assigned him as second astronomer. He enriched the Ephemerides with a great number of important papers. and published a memoir *On the relation between comets, shooting stars, and meteors* (1873), a well-known paper, which threw great light on a vast subject which the author still follows up. Observations of stars between 105° and 115° p.d. were continued, and the results published by Capelli in 1865. In 1875 the *Effemeridi di Milano*, which had been carried on for a century, were now become of little use, owing to the ready access which facilities of communication gave every one to the *Nautical Almanac* and the *Connaissance des Temps*. In place of needlessly spending the strength of the observatory in superfluous calculations, M. Schiaparelli determined rather to devote it to the extension of the field of his observations. Since that date the publications of the observatory of Brera have become irregular, and only contain special memoirs. Amongst these is Schiaparelli's memoir on the Comet III. of 1862, associated, as is well known, with the swarm of the Perseids. Observations were made in 1870 by MM. Schiaparelli and Celoria, Hirsch, and Plantamour, for connecting the Italian triangulation with that of Switzerland and Austria, the operations in 1822-23 not being considered sufficiently accurate. Milan was connected with Switzerland by a direct telegraphic determination of the difference of longitude between the College of Brera and the summit of the Simplon. The difference of longitude between Milan and Brera was found 7m. 0s., 183, with a probable error of 0·013s. After this the astronomers of Brera resumed their ordinary labours, the principal of which was the places of stars from first to eleventh magnitude between 15° and 25° S. dec., which is still continued by Celoria. The equatorial of Sisson is now replaced by another by Merz, with a telescope of 8.5 inches aperture and 10½ feet focal length, moved by clockwork; the circles read to 10″, and the objective bears a power of 700. When M. Rayet was at Milan, in October, 1876, this instrument was being carefully tested, and M. Schiaparelli proposes to make with it a series of observations on double stars, for which the transparency of the Italian sky will be favourable.

The traditions of the Brera Observatory, from its humble origin to the present time, are highly honourable to Italy; and in the experienced hands of MM. Schiaparelli and Celoria, its fine instruments will not fail to be turned to account.

FLORENCE.—In 1774 Leopold, of Austria, Grand Duke of Tuscany, caused an observatory, together with a great collection of objects relative to the physical sciences and natural history, to be established in a portion of the Pitti Palace; but it was not before December, 1784, that a transit instrument 3·8-inch aperture, and 7·8 feet in length, and a zenith sector with an objective of 4-inch diameter and 9 feet in length, both by Sisson, were placed in position by Slop, of the Pisa Observatory. No observer was appointed till March 14th, 1807, when Domenico de Vecchi found the instruments in a wretched state. One of his first objects was the determination of the geographical position of the observatory, after which, with his assistant, Cosmo del Nacca, he made some observations of stars near the zenith, which the imperfections of the zenith sector soon arrested. At last, on 5th March, 1824, a large repeating circle arrived from Munich, but Vecchi was already very old, and he died in 1829, without effecting any work of importance. He was followed by Pons, formerly of the Marseilles Observatory, and celebrated for his numerous discoveries of comets (forty-five),[*] but who died soon after, October 16th, 1831, and the Grand Duke of Tuscany called from Modena, as his successor, J. B. Amici, already well known for his optical inventions. Amici was born at Modena in 1786. After studying at Bologna he was appointed to the chair of algebra and geometry at the Lyceum of Modena, and to the directorship of the observatory of Florence, on 15th March, 1831. With the eye-piece micrometer of his own invention he made a series of measurements of the sun, moon, and double stars, which unfortunately have not been published, and some observations on Jupiter's satellites in full daylight, which appeared in the Correspondence of the Baron de Zach. In 1840 he purchased two discs of flint and crown glass 11-inch diameter, and caused them to be figured under his direction, but the telescope was not finished and set up till 1854, and from that time to 1863 Amici employed it for observations of comets, amongst them the great comet of 1858 (Donati). Amici died at Florence, April 10th, 1864.

Donati, who had been his assistant, during the latter years of his life, succeeded him. Born at Pisa in 1826, he studied under Mosotti, from whom he derived the love of astronomy. An ardent observer, the young Pisan discovered successively 6 comets, including the great comet of 1858, which bears his name, and observed and calculated the orbits of the principal ones that from time to time appeared. But his great merit was the study of the spectra of stars, for which he used at first the great lens, 16-in. diameter, given by Cosmo de Medici to the Academia del Cimento; and about the end of 1857 he began to compare the spectra of stars of the 1st magnitude with that of the sun; Amici greatly helping him by placing at his disposal the first direct vision prism that had been constructed. His memoir of 1860 gives a description far more complete than Fraunhofer's of the spectra of thirteen stars of the 1st magnitude. We owe to him likewise an interesting memoir on the solar eclipse of July 18, 1860, which, in company with other Italian astronomers, he observed in Spain. The position of the observatory of the Museum was unsuited for exact observations, and at the meeting of the Geodesic Conference, at Berlin in 1864, Donati was obliged to confess that, like the greater number of those in Italy, the observatory of Florence could not be employed for observations for a catalogue of stars. Nevertheless, as the Museum had made the acquisition of the large telescope of 10½ inches, by Amici, it was imperative to find a site for it. Finally Donati proposed the transfer of the whole observatory to the hill of Arcetri, in the vicinity of the ancient abode of Galileo. By the generous concurrence of the Municipality, the Provincial Council, and the Government, a sum of 106,000 livres (£4,240), afterwards increased to 136,000 (£5,440), was collected for the construction of a new edifice. Begun in September, 1869, on the plans of Donati, the work was completed in the autumn of 1872, and on October 28th of the same year the observatory was inaugurated with great ceremony. In the centre, beneath a great cylindrical cupola that surmounts the building is placed the telescope of Amici, the mounting of which is still unfinished, and the circles not yet graduated. A spectroscope of twenty-five prisms had also been procured. The lamented death of Donati, by cholera, on the 19th September, 1873, left unfinished the most important work of his life. M. Schiaparelli was offered the direction of the observatory, but he declined it, and sent his most active assistant, M. W. Tempel, who in Venice, Marseilles, and Milan has discovered so many planets and comets. M. Schiaparelli also exerted himself to obtain the completion of the establishment, and the supply of other needful instruments; but hitherto it would appear, without success. The observatory was described by a visitor in January, 1878, as being sadly out of repair, the rain let in in many places; whilst the indefatigable M. Tempel was working at his review of the principal nebulæ, of which he was making fine drawings with Amici's glass, but without sufficient eye-pieces, or clockwork movement, and so defectively mounted that it could not reach a large part of the heavens. And the Senator Antonio Berti, in his address to the

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[*] Chambers's Catalogue in his Descriptive Astronomy, marks 35 comets discovered by Pons, of 28 of which he was the first discoverer.

Senate, July 1st, 1878, on behalf of the Arcetri Observatory, gave a mournful description of its ruinous condition, injurious as well to science as to the credit of the nation, and filling with regret strangers who visit a site with such historical memories, and a kind of terrestrial paradise from its beauty, and the splendour of the sky which covers it. Signor Berti proceeded to state that a sum of from thirty to thirty-three thousand lire (£1,200 to £1,300) would suffice for repairs and the more urgent wants. His speech was received with general approbation; but as yet, it seems, no action has followed. We wish we were rich enough to make an offer of the sum. Papers by M. Tempel will be found in the *Astronomische Nachrichten*, Bd. 90, Nos. 2138—39, and Bd. 93, No. 2212, and other numbers. See also *The Observatory*, vol. i., p. 403. His solitary position, and the disadvantages under which he is working deserve much sympathy.

We are surprised that M. Rayet makes no mention in his valuable work of the observatory of the Scuole Pie, at Florence, where in years past such good work was done by Inghirami and others. (See *Astronomical Register*, vol. ix., p. 186, 1871.)

Observations made during Total Solar Eclipses. Collated by A. C. Ranyard, M.A., Sec. R. A. S. Memoirs of the Royal Astronomical Society. Vol. XLI. 1879. With eighteen plates. pp. viii. 792. London: published by the Society at their apartments, Burlington House.

It is fortunate that this extensive collation of observations of total solar eclipses was undertaken by so competent a person as Mr. Ranyard. It must have been a work of great labour, demanding much patience, industry, and judgment, and a practical as well as theoretical familiarity with the complex subject. Nothing like it has ever appeared before. The index of the MS. and printed accounts of solar eclipses which have been referred to itself occupies 36 pages. It is quite a mine of wealth for students of solar phenomena, and must always remain of value. Of the eclipses described the earliest is that of May 1, 1715, and the latest that of April 5, 1875. There is also a print of the corona of the eclipse of July, 1878, of which, however, the observations have not been collated. Mr. Ranyard expresses his obligations in various ways to the Astronomer Royal, to Lord Lindsay, to Mr. Wesley, and Mr. Marth, and for the loan of books, MSS., and photographs to Padre Secchi, Professor Argelander, Dr. Schellen, Professor Young, Professor Winlock, and others. Besides the 18 fine plates at the end, there are woodcuts in unsparing profusion throughout the volume, which contains an alphabetical list of the places of observations (latitude and longitude) and observers, and also a general index. Many of our readers are perhaps not likely to possess this work, and we think we cannot do better at the outset than transcribe the table of contents, as being a kind of introductory chapter, and an interesting résumé of the many points connected with the grand phenomenon of a total eclipse.

There are xliv. chapters. I. Observations of the occultations of sun spots by the limb of the moon. II. The moon seen to be darker than sun spots. III. Band or fringe along the moon's limb. IV. On the spectroscopic and photographic evidence of a fringe along the moon's limb. V. Part of moon's limb seen outside the sun's disc. VI. The whole contour of the moon seen before or after totality. VII. The cusps of the solar crescent seen as coloured. VIII. The moon seen as red before totality. IX. Shadow bands. X. Pulsations of light on approach of totality, and tremulous motion of thin crescent. XI. Faint brushes of light from the solar crescent. XII. Parhelia and rays seen just before totality. XIII. Brushes of light along the edge of the disappearing crescent. XIV. The corona seen before and after totality. XV. Stars seen before totality. XVI. Prominences seen before and after totality. XVII. Baily's beads. XVIII. Swimming round and flowing together of Baily's beads. XIX. The moon and coronal rays seen to rotate. XX. The moon appears to jump forward at the beginning and end of totality. XXI. The chromosphere seen as a continuous red arc. XXII. Telescopic observations of Young's stratum. XXIII. Double observations of the commencement and end of totality. XXIV. Approach of the moon's shadow seen at totality. XXV. The heavens appear to descend at totality. XXVI. Colours on clouds. XXVII. Iridescent clouds and arcs of prismatic colour. XXVIII. Colour of the sky. XXIX. Appearance of the moon's disc during totality. XXX. Stars seen during totality. XXXI. Brightness of the horizon during totality. XXXII. Darkness during totality. XXXIII. Unnatural illumination of objects during totality. XXXIV. Flashes of light seen near the moon's disc. XXXV. Wind during totality. XXXVI. Depression of temperature and dew during totality. XXXVII. Effect of eclipse on men, animals, and plants. XXXVIII. Magnetic observations during eclipses. XXXIX. Trou d'Ulloa and notches seen in the limb of the moon. XL. Zodiacal light and Intra-Mercurial planets searched for during totality. XLI. Brightness of the corona. XLII. Polariscopic observations. XLIII. Spectroscopic observations. XLIV. Photographs and drawings of the corona. Mr. Ranyard prefaces each of the chapters with some remarks, and then gives the original communications of the different observers—English, American, French, Italian, German, Spanish, in the order of their dates, and to those in foreign languages he puts a summary in English in the margin.

We now proceed to note a few interesting particulars pertaining to the different chapters. I. No distortion in the sun spots produced by a lunar atmosphere has been detected: a few appearances to the contrary seem to admit of another explanation. II. "It should be remembered that during the partial phases of an eclipse the atmosphere between the observer and the sun is brilliantly illuminated, and, added to this, the moon is exposed to full or nearly full earthshine. The moon, nevertheless, appears as a deep black compared with the darkest parts of the sun spots." III. Fringes of colour, and dark and light bands, have been noticed by various observers along the part of the limb of the moon which is projected upon the sun's disc during the partial phases. These cannot all be accounted for by imperfections in the object glass or eye-piece; nor as due to an effect of contrast between the light and dark discs of the sun and moon. IV. Examination of the solar spectrum in the neighbourhood of the moon's limb has not detected any additional lines, nor change in the ordinary lines: the bright band along the moon's limb, which has been seen with the telescope, and appears in the photographs, has been ascribed to an effect of contrast, or else to an increased deposit of silver on the negative. V. "The evidence is somewhat conflicting as to whether any portions of the moon's limb can be traced beyond the sun's disc, except during the few minutes immediately before the commencement and after the end of totality: when . . . there is little doubt that the moon's limb can be seen projected on the corona." "It has been suggested that, although the corona in general may not be sufficiently bright to form a background on which the moon's limb can be distinguished, yet that as the moon in its passage across the sun passes over prominences, and even brighter parts of the corona, it may be rendered visible, and may again be lost." Only one observer, however, mentions the precautions taken to avoid being biassed by the presence in the field of the contiguous portion of the moon's limb projected upon the photosphere. VI. "Observers differ greatly as to the length of time during which the disc of the moon can be traced, before or after the total phase." Thus Arago (1842) saw it 20m. before totality: Bouvard (1842) 38m. after totality: Hind (1851) about 25m. before totality: Pettersson (1851) 15s. after totality. VII. Some observers have seen both cusps near the time of total obscuration of a reddish colour, and others the whole exterior limb of the crescent as of a tender rose colour: Cacciatore describes the crescent as of a violet tint, tending to red towards the cusps. VIII. The moon's colour before totality has been described as a deep crimson glow—a dull coppery red, a reddish brownish light, a dull glowing coal. IX. Some observers describe the shadow bands as being visible for a short time both before and after totality. Some persons speak of them as visible during totality. They have been observed during an annular eclipse, and have been seen outside the path of totality. "The time at which they have been perceived before totality, and during which they have remained visible after totality, varies with the different observers from a few seconds to as much as five minutes." . . "they are frequently described as being similar in appearance to the moving lines of light and shade which may be seen when sunlight is reflected from the surface of rippling water on to some adjacent wall." . . . "They are usually described as wavy lines of light and shadow moving rapidly forward over the surface of the ground." Some describe the patches or bands as coloured; all the other observers (1851) speak of them as shadows separated by brighter interspaces. "The rate at which the shadows move over the ground has been variously estimated. It appears certain, that the rate of their motion is not to be compared with the velocity with which the moon's shadow travels over the earth." . . . "The direction of the motion of the shadow bands does not appear to coincide with the direction of the motion of the moon's shadow over the earth, nor does it appear to have any constant relation to this direction." Information is wanting as to the direction of the wind at the stations where the bands have been observed. Three sketches of these bands are given; in one of which they are represented as moving rapidly over the front of a house, when they were about eighteen inches in breadth. X. The limbs of the solar crescent are described by some observers as appearing to undulate violently on the approach of totality. The phenomenon has been perceived six and eight minutes before the commencement of totality. Others speak of the undulations as being visible for only a few seconds before or after totality. The following appearances are also recorded:—undulating red rays thrown out by the sun just before and after totality; undulations seen in the air, which appeared to go in the same direction as the wind. XI. "These rays are in each instance described as springing from the cusps of the crescent, and appear not to have been very conspicuous objects." . . . "The more conspicuous rays seen in the few minutes preceding or after totality are not always described as springing from the cusps; they are drawn by some of the observers as springing from several points of the limb of the solar crescent." . . . they have been described as tangential and stretching in both directions. In length they are described as equal to the diameter of the moon,—one degree,—more than 3′. In appearance and brightness, like the tail of a comet—brushes of electric light, sunbeams slanting through a cloud, a delicate and exquisitely defined little ray or cone of light. By some observers the rays are described as changing in form, or growing in length or distinctness during the time that that they were observed. "The time at which the rays or brushes were first perceived before totality, or during which they have been traced after totality, appears to vary from a few seconds to nine minutes." Many drawings of these rays are given. Professor Stokes, in a note, observing that "the slender beams of light or shade shooting out from the horns of the crescent would seem to admit of easy explanation, supposing them to have been of the nature of sunbeams, depending upon the illumination of the atmosphere of the earth by the sun's rays," proceeds to explain the theory in detail. Mr. Ranyard points out a difficulty in the way of it. XII. "The appearances [parhelia and rays] described in the two following extracts seem to have been of the same ghost-like character with the rays and brushes of light described in the last chapter. Though more complicated in form, they appear, like the rays and brushes, to have been symmetrically situated with regard to the solar crescent, and to have disappeared on the commencement of totality." They were seen (1860) by Arndt and Mädler, and a sketch is given. XIII. The brushes of light seen along the convex limb of the disappearing crescent have been seen for nearly 2 minutes to a few seconds before totality. Two drawings accompany the accounts. XIV. "The corona is in all cases described as becoming visible on the side of the dark moon opposite to the disappearing crescent. Oom speaks of it as *suddenly* forming round the moon as a ring of light about 3′ broad, fading towards the outside, at first concentric with the moon, but afterwards with excrescences. Jacob describes it as 'flashing out suddenly,' and spreading 'almost instantly round the circumference like the opening of a fan.' . . . On the other hand, Beck says that the corona seemed not so much to be formed, as to be rendered visible by the diminution of light; and of the observers who watched the disappearance of the corona after totality, Thomson says that it was gradually rendered invisible, and Lockyer that it faded away in the rapidly increasing sunlight. It should be noticed that none of the three observers who describe the *sudden* appearance of the corona mention that they had previously been looking for it in vain. Beck, Pole, and Secchi describe the traces of the corona, seen by them while the solar crescent was visible, as being so bright as to be easily seen through a dark glass. On the other hand Pogson states that he saw no trace of the corona after totality, though he looked for it both with the naked eye and with the telescope. The time during which the corona has been perceived before and after the total phase varies from a few seconds to as much as twelve minutes." A sketch is given by Dawson, who observed the rays as suddenly springing up along the arc of the black moon.

XV. "Only bright stars such as Venus, Jupiter, Mercury and Sirius are, it would appear, visible before totality. Venus, which, it should be remembered, has frequently been observed in full daylight, was perceived in the eclipse of 1715, when the sun was three-fourths eclipsed; Jupiter has been detected five minutes before totality, but Martin Saar, who observed it, knew pretty accurately the part of the heavens in which to look for it." XVI. "The time during which prominences have been seen before and after the total phase varies from a few seconds to about six minutes." XVII. "The length of time during which Baily's Beads have been noticed as being visible before the beginning and after the end of totality has been variously estimated. One would have expected that the time of their duration would have chiefly depended upon the height of the mountains which happened to be upon the moon's limb at the time of the eclipse; and that for stations situated near the central line of totality, the estimates of the time of their duration would not greatly differ. But such does not appear to be the case." They have been noticed from 3 to 15 and 40 seconds before totality. Baily, whose account, 15 May, 1836, is given, with 4 diagrams, speaks of the phenomenon as lasting for only "six or eight seconds, or perhaps ten at the utmost." At the commencement of a total eclipse the dark lines across the solar crescent are seen to grow shorter and broader, and the intervening bright spaces assume rounded forms which have been compared to a luminous string of beads,—irregularly shaped fragments which at no time resembled beads,—diamond-shaped beads, which appeared to adhere to each other and to the limbs of the sun and moon,—string of brilliants which disappeared like snow under a white heat, etc. "Many of the observers describe the beads or intervening dark spaces as merging or flowing into one another like drops of water." Baily at his second observation in 1842 failed to see the black lines, although he caught sight of the beads. Struve in 1851 saw neither black lines nor beads, although he carefully looked for them. XVIII. "Of the observers who describe Baily's beads as appearing to move, there are some who speak of the whole line of luminous points as swimming, waving, or moving together as a whole; while others describe the phenomenon as if only the separate beads of light changed form or flowed into one another." Some observers describe the beads, and some the dark intervals, as running or flowing into one another like drops of water or globules of mercury. Dunkin (1851) and Tennant (1871) are very definite in their assertion that there was no running or flowing together of the beads during their disappearance. The latter says "they were clearly small specks of light passing hollows in the moon's limb, and enlarged by the irradiation of the telescope." XIX. "The appearance of rotation described in the following extracts is not confined to the first few seconds of totality, though Wyberd and Brogi describe the phenomenon as if it was especially visible just after the moon's disc had appeared to rush forward on covering the sun. The other observers speak of the corona as if it continued to revolve." Ulloa (1778) says, "Five or six seconds after the immersion we began to observe round the moon a long brilliant circle of light which seemed to have a rapid circular motion, something similar to that of a rocket turning about its centre." XX. "Wyberd and Baynes describe the even motion of the moon as appearing to be accelerated at the moment when the lunar limb blotted out the last trace of the photosphere." In a note Mr. Ranyard remarks that, "observers had repeatedly noticed that the solar crescent appears up to the last moment before its extinction as a band of sensible width, and the sudden disappearance of such a comparatively broad line will, no doubt, account for the apparent acceleration alluded to." XXI. "Towards the end of totality the prominences upon the sun's western limb appear to increase rapidly in number, and at length they are seen to be joined together at their base by a streak of red light which has in some cases been traced extending over an arc of more than 60° along the moon's limb. Stannyan speaks of the blood-red streak seen by him as continuing to be visible not longer than six or seven seconds of time. Halley says two or three seconds, and Bullock two seconds. A similar red arc is also visible at the commencement of totality. Littrow describes that seen by him just after the sun's disappearance as 0′·2 broad." "The red arc must (at all events in some eclipses) be conspicuously visible. It is intensely bright compared with the corona; indeed, Bullock seems to have mistaken it [at first] for the reappearing sun." XXII. "Schmidt, Fearnley, Secchi, and Tacchini have observed that the lower layers of the chromosphere which are uncovered by the moon's limb, in the few seconds immediately preceding the end of totality, appear of a decidedly whiter hue than the prominences and upper parts of the chromosphere which have been previously uncovered. Schmidt describes the red arc of the upper chromosphere as appearing to be separated from the dark limb of the moon by an intensely shining, silvery-white line of light—which has, no doubt, frequently been mistaken for the returning light of the photosphere; and hence, probably, the double observations of the commencement and end of totality described in the next chapter." . . . "The number of bright lines visible in the lower chromosphere is almost innumerable," XXIII.—"the brightness of the solar envelopes immediately surrounding the photosphere is such that observers have frequently been puzzled in determining the exact moment of the commencement or end of totality." According to some observations "there would appear to be a bright layer above the photosphere with a defined upper limit." But according to others, "the degradation in brightness from the photosphere outwards is perfectly gradual. It is possible (and from spectroscopic observations it appears probable) that the bright layer at the base of the chromosphere may differ materially in thickness at different eclipses. . . in 1851 the interval between the two signals for totality of Robinson was 5 sec. In 1868 the interval between the two sets of observations at Wha-Tonne was 12 sec."

XXIV. "The rate at which the shadow of the moon travels over the earth's surface is so great that few observers except those situated upon high ground overlooking a great extent of country, have been successful in observing it in its passage over the earth; but as seen in the air, or travelling over distant clouds, its motions can, it would seem, be easily followed. Forbes, Airy, J. Beck, and W. Beck, Miss Airy, Grant, and Ranyard describe the shadow of totality seen in the distance as appearing like a dark storm upon the horizon. Airy, Platt, Mrs. Farrell, Archibald, Hill, and Samuelson describe the shadow as visible in the air. Mrs. Farrell speaks of the coming darkness as appearing to rise from the horizon, "like the lifting of a dark curtain." . . Platt describes the appearance of the shadow in the air as like a dark column or very dark cloud, and Forbes speaks of the approach of the "colonne ténébreuse," Nones describes the shadow as of a "dark rose colour," and Hill refers to the "deep violet shadow." Lady Airy and Winnecke speak of the shadow as appearing like smoke, Mentesino describes it as a "wall of fog," Samuelson as a "vaporous shadow," and Perowne says that it was neither like shadow nor vapour. The same appearance is no doubt referred to by Airy in 1851, who speaks of a "darkness in the south-east," which he immediately perceived to be the eclipse shadow. Forbes was so confounded by the frightful velocity with which the shadow swept over the earth towards him [calculated by Carlini for Turin (1842), an Italian mile and a-half per second], that he felt as if the great building on which he was standing was commencing to fall over in the direction of the coming darkness. Biddulph describes the rapidity of the motion of the shadow as "perfectly frightful"; he involuntarily listened for the rushing noise of a mighty wind. Platt talks of the "frightful rapidity" with which the darkness advanced, and Eastman speaks of "the rush of a peculiar, almost tangible darkness." On the other hand Hill says that the motion of the shadow was "much slower and more majestic" than he had been led to expect. Silverstolpe, Grant, Myer, Lindsay, Ranyard, and Lane, or those with them, failed to observe the passage of the shadow, although their attention was specially directed to looking for it. Krag, Ragoonatha Chary, Hill, and Myer all noticed that the darkness did not commence instantaneously with what appeared to them to be the time of the passing of the shadow, but that the darkness gradually increased until in a few seconds it appeared to be at its height. This may possibly be accounted for by the illumination which would be derived from the clouds and sky in the neighbourhood not yet immersed in the moon's shadow, and does not necessarily indicate that the light of the solar crescent was only gradually extinguished."

(*To be continued.*)

REVIEW.

Sun, Moon, and Stars. A book for beginners, by Agnes Giberne, with a preface by the Rev. C. Pritchard, M.A., F.R.S., &c.; Savilian Professor of Astronomy in the University of Oxford. Seeley, Jackson, and Halliday, 54, Fleet Street, London. 1880. pp. 302. 5s.

We have read this little work with interest and pleasure, and we think that it is not merely *a*, but *the* book for beginners, and this not to the disparagement of various nice introductions which have before appeared. Each has its merits, but upon the whole a more easy and pleasant first book on astronomy could not be read in schools, or by any persons of either sex. It is not merely a compendium, but it has a *roundness* and *character* that evince in its authoress a familiarity with the subject, and the faculty of using the abundant material of late years with judgment and discrimination. In all that comes within its scope it seems to be up to the present state of the science; it has pretty illustrations, and we see with much satisfaction each chapter headed by an apposite passage from Scripture, and references to the Great Creator and Upholder of all things are not wanting in the text when such seem particularly natural. It is written in a clear and lively style; the type, paper, and binding of the book are very good, and its price is moderate. We assume that the numerical statements are generally correct, but we noticed a slip on page 129, where the speed of movement of a solar spot is represented as having been *three* times faster than the movement of clouds in the most violent hurricane in our globe. It would, we fancy, be nearer the mark to say 13,000 times faster. There may have been some confusion here between feet and miles in the computation. The authoress is careful to distinguish between the certainties of astronomy and various questions more or less involved in the haze of speculation. The following, however, appears to us an exception to this generally cautious proceeding:—"Step by step we have advanced, from the first early notion of our earth as the mighty centre of the universe, to a stage where earth and planets vanish from sight, and the great central sun of our system himself shines forth but as one glimmering point of light amid a million of stars." p. 294. Would, indeed, that we had got so far as to be able to make this last assertion. Indeed, in other parts of the book the subject is treated with much more reserve. p. 282. "How little we know about the matter! only so much can we tell,—that the controlling and restraining Hand of God is over the whole. Whether by the attraction of one great centre, or by the united influences of a thousand fainter attractions, He steers each radiant sun upon its heavenly path, 'upholding all things by the word of His power.'—Heb. i. 3. There is no blundering, no confusion, no entanglement. All is perfect order, calm arrangement, restrained energy." p. 242—43.

This is true and well expressed, but we would suppress the alternative. It is little indeed we know about the matter, but we are disposed to agree with those who think that little rather points to a period, however distant, when, unless some fresh adjustment is made, the entire universe would be involved in ruin. We may be certain that it will last as long as it was intended in its present state; and very likely such an adjustment, so to call it, may be made long before the catastrophe can take place. The study of such a chart of stellar proper motions as Mr. Proctor gives in his "Universe of Stars," with arrows pointing in all directions, some of enormous length, and these representing motions only in the plane of the paper, is not suggestive of the delicate balance of the universe preserved by faint and distant attractions. On this subject Prof. Newcomb has some interesting remarks in his "Popular Astronomy," pp. 409, 483—90.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

OBSERVING WEATHER.

The following notes on the observing weather of places differing considerably in longitude and latitude may interest and console those who are suffering from the continued bad weather of 1880.

In vol. 9 of the Dorpat Observations Mädler gives the detailed results of his double-star measures for 1841. It appears that he measured about 170 pairs, the number of angles being about 2,350 and the distances 1,500. A few stars were measured on but one night, most of them on about three, and several on nine or ten nights. In this year he measured double stars on five nights in January, three in February, three in March, fourteen in April, fifteen in May, eighteen in June, seventeen in July, twelve in August, five in September, two in October, (none in November), and one in December:—Total, 95. Possibly he did other work on some other fine nights. At any rate the year was certainly not a bad one. He observes that in 1844 out of 132 nights, or about a third of the year, there were but eight really clear.

OΣ in his "*Observations de la Grande Nébuleuse d'Orion*" has the following remarks on the weather:—"During these six months [the middle of September to the middle of March] the number of nights effectively favourable to the observation of objects demanding a certain degree of steadiness is extremely limited in our climate [*i.e.*, at Poulkova, near St. Petersburg]. On the average the number of good nights may be put at about half-a-dozen during this period of the year." He goes on to remark that the best nights occur towards the end of September and in the beginning of March: in full winter the number of clear nights is very limited, and the usefully clear nights extremely rare; and owing to the great cold, stars which can be readily measured in strong twilight cannot even be seen double when full night sets in.

Mr. Edward Crossley's Observatory: J. GLEDHILL.
Bermerside, Halifax.

THE RED SPOT ON JUPITER.

The red spot on the southern hemisphere of Jupiter is still there, and so far as can be judged from one observation it is not much changed. The weather did not permit me to obtain an observation earlier than the 9th June, 15h. 30m. Berlin mean time; and at that time nothing could be seen of the red spot. Then followed seven days when the sky was overcast; on the 16th June it was clear; however, the planet presented to the earth the same side as on the 9th June, so that at 15h. 30m. nothing could be found.

On the 17th June I hoped positively to have seen something of the red spot, but at 15h. 0m. no trace could be noticed, and I began to fear that the interesting object had disappeared since the date of my last observation on the 7th February, 1880. For three-quarters of an hour I watched the disc with attention, during which time the weather became very bright; I then noticed at 15h. 50m. a faint shadow on the eastern limb; at 16h. 8m. it had advanced so far upon the disc that I could not doubt any more that the red spot was before me, but the daylight becoming stronger prevented my further observation. I calculated approximately for the next day, the 18th June, the time of the transit of the red spot across the centre of the disc, but again dull weather prevented observation. At last, on the 20th of June, at 14h. 22m Berlin mean time, the planet could again be seen, with favourable weather. The red spot was still there, and apparently little or not at all changed; the forward end had not yet reached the middle of the disc, so that before daybreak two good observations of the beginning and middle of the spot might be expected. I observed with the large 12-inch refractor of the Potsdam Observatory, and made use of a thread micromoter in order to determine the time of the transit more exactly, if possible, than by a mere estimate. A thread was placed perpendicularly to the belts of the planet, and from time to time the position of the spot with respect to the illuminated limbs of the planet was measured, and I obtained the following times for the transits of the beginning and middle of the red spot.

	Beginning of red spots.	Middle.
1880, June 20.	14h. 5m. 31s.	14h.32m. 31s.
	Greenwich M. T.	

With respect to the general appearance of the planet it is now very uniform in colour (*sehr eintönig*), and the belts are much reduced.

O. LOHSE.

SPLENDID METEOR.

Coming home from the opera on Saturday, the 17th, at 4m. past 12 p.m., as I was passing up Baker Street, my attention was arrested by what appeared to be a very vivid flash of lightning, but as the light continued I raised my eyes to the quarter whence it came and saw a magnificent sight—a great fireball had burst, apparently in the constellation Andromeda, with a trail of light for a long distance nearly parallel beneath the two upper stars of the square of Pegasus. Both the trail and nucleus were visible for several seconds.

F.R.A.S.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN AUGUST, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Spots on Plato.—A new series of observations of the spots and streaks on the floor of Plato is now in progress, and as it is probable that some important results may be obtained, it appears desirable that more observers may join in the work. With this view the following remarks may not be inappropriate, as indicating the objects to be attained, and the best means for attaining them:

First, the objects to be attained. These are the connections between the spots and the streaks. In the year 1872 it was suggested that some of the spots on Plato were volcanic cones, that the increase of visibility of spots indicate that such spots are in an active state, and that this state of activity is still further to be inferred from streaks being formed in connection with spots, the visibility of which has been found to increase. If these conditions are observed to obtain through at least twelve or thirteen lunations, it may safely be inferred that such a connection exists.

Second, the means of attaining this result. On every occasion that *Plato* can be telescopically observed, the spots seen should be noted, for example, as 1, 4, 3, 17, 5, 22, 14, 13, 16, &c. Note also if any streaks are associated with any spots as "streaks seen between spots No. 5, 14, and 17." An account should be kept of these observations and as soon as convenient after sunset on *Plato* all the observations during the lunation should be forwarded to W. R. Birt for examination and discussion in a schedule of the following form:

		1880.			
Jan. 25	Jan. 26	Jan. 27	Jan. 30		
1	1	1	1	=	4
3	3	3	3	=	4
4	4	4	4	=	4
	5	5	5	=	3
	14	14	14	=	3
	16	16		=	2
22	22	22	22	=	4
		7		=	1
		17		=	1
4	7	9	6		26

The table shows that on Jan. 25 four spots were seen, viz, 1, 3, 4, 22. On Jan. 27 nine spots were seen, viz., 1, 3, 4, 5, 14, 16, 22, 7, 17. The last column shows there were four observations of No. 1, four of No. 3, four of No. 4, and so on, there being 26 observations in all. The last line shows the number of spots observed each day, the total number of the last line and the last column *agreeing*.

If any doubt arises as to the identity of one or more of the spots a diagram should be forwarded with the schedule. A note of interrogation should never be inserted against the number of a spot in the schedule, the observation being useless, if any doubt attaches to it, but a note of interrogation may be inserted in the diagram against the number, as 13? if any doubt exists.

Corinium House, Romford Road,
Stratford, E.: July 15, 1880.

SCHAEBERLE'S COMET.

1880.		R.A.			δ		Light.
Aug. 4	...	6	58	29	... +32	9	
8	...	6	59	13	... 31	4	0·70
12	...	6	59	44	... 29	57	
16	...	7	0	1	... 28	49	0·74
20	...	7	0	2	... 27	40	
24	...	6	59	46	... 26	29	0·77
28	...	6	59	11	... 25	16	

The unit of light is the comet's intensity on April 6. From the above it will be seen that the comet is increasing in brilliancy, and it will continue to do so till Oct. 27, when the brilliancy will be 1·21.

ASTRONOMICAL OCCURRENCES FOR AUGUST, 1880.

DATE.		h. m.	Principal Occurrences.	Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m. Altair.
Sun	1		Sidereal Time at Mean Noon 8h. 41m. 52·94s.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	12 4 13 24 14 19 15 37	— 11 1·3
Mon	2		Saturn's Ring: Major axis=41″·30 Minor axis=11″·15	3rd Tr. I. 1st Oc. R. 3rd Tr. E.	10 49 12 45 13 6	10 57·4
Tues	3		Sun's Meridian Passage 5m. 53·96s. after Mean Noon	1st Tr. E.	10 4	10 53·4
Wed	4	20	Inferior conjunction of Mercury and Sun	2nd Sh. I.	16 24	10 49·5
Thur	5	15 48 12 23	● New Moon Conjunction of Moon and Mercury 1° 46′ S. Neptune at opposition to the Sun			10 45·6
Fri	6	9	Conjunction of Moon and Venus 5° 27′ N.	2nd Ec. D.	11 32 38	10 41·6
Sat	7					10 37·7
Sun	8	1	Conjunction of Moon and Mars 6° 13′ N.	2nd Tr. E. 1st Sh. I. 1st Tr. I. 1st Sh. E.	10 52 13 58 15 13 16 13	10 33·8
Mon	9			1st Ec. D. 3rd Sh. E. 3rd Tr. I. 1st Oc. R.	11 9 12 7 14 34 14 34	10 27·8
Tues	10			1st Tr. I. 1st Sh. E. 1st Tr. E.	9 40 10 41 11 54	10 23·9
Wed	11					10 19·9
Thur	12					10 16·0
Fri	13	0 42	☽ Moon's First Quarter	2nd Ec. D.	14 7 57	10 14·1
Sat	14					10 10·2
Sun	15		Illuminated portion of disc of Venus=0·988 Illuminated portion of disc of Mars=0·986	2nd Tr. I. 2nd Sh. E. 2nd Tr. E. 1st Sh. I.	10 40 11 3 13 17 15 52	10 6·2
Mon	16	6 22 7 33	Occultation of 6 Sagittarii (6) Reappearance of ditto	1st Ec. D. 3rd Sh. I. 3rd Sh. E. 1st Oc. R.	13 3 16 13 21 16 7 16 23	10 2·3
Tues	17		Sidereal Time at Mean Noon 9h. 44m. 57·82s.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	10 21 11 29 12 36 13 42	9 58·4

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Altair. —
Wed	18		Sun's Meridian Passage 3m. 33·23s. after Mean Noon	1st Oc. R.	10 50	9 54·4
Thur	19	17 18	○ Full Moon			9 50·5
Fri	20			3rd Oc. R. 2nd Ec. D.	9 58 16 43 10	9 46·6
Sat	21	7 43 9 16 9 50 9 22 14 33 15 30	Occultation reappearance of B.A.C. 8152 (6½) Occultation of 9 Piscium (6) Reappearance of ditto Near approach of κ Piscium (4½) Occultation of 16 Piscium (6) Reappearance of ditto			9 42·6
Sun	22	14 9 15 24	Occultation of 45 Piscium (6) Reappearance of ditto Saturn's Ring: Major axis=42″·75 Minor axis=11″·46	2nd Sh. I. 2nd Tr. I. 2nd Sh. E. 2nd Tr. E.	10 56 13 4 13 39 15 41	9 38·7
Mon	23	17	Conjunction of Moon and Jupiter 6° 56′ S.	1st Ec. D.	14 57 37	9 34·8
Tues	24	11	Conjunction of Moon and Saturn 7° 42′ S.	2nd Oc. R. 1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	10 39 12 15 13 17 14 30 15 29	9 30·8
Wed	25	15 53	Near approach of 47 Arietis (6)	1st Ec. D. 1st Oc. R.	9 26 11 12 37	9 26·9
Thur	26			1st Sh. E. 1st Tr. E.	8 58 9 55	9 22·9
Fri	27	4 14	☾ Moon's Last Quarter	3rd Ec. R. 3rd Oc. D. 3rd Oc. R.	9 46 44 11 20 13 29	9 19·0
Sat	28	15 32 16 38	Occultation of 121 Tauri (6) Reappearance of ditto			9 15·1
Sun	29	13 46	Near approach of μ Geminorum (3)	2nd Sh. I. 2nd Tr. I. 2nd Sh. E.	13 33 15 26 16 16	9 11·2
Mon	30			1st Ec. D.	16 52 5	9 7·2
Tues	31			2nd Ec. D. 2nd Oc. R. 1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	8 35 54 12 58 14 9 15 3 16 24 17 13	9 3·3
SEP	T.					
Wed	1	0	Conjunction of Uranus and Sun	1st Ec. D. 2nd Oc. R.	11 20 41 14 24	8 59·4

THE PLANETS FOR AUGUST.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	9 8 27	N.11 20	11″·2	0 26·5
	9th	8 44 5	N.13 55½	10″·4	23 26·8
	17th	8 43 5	N.16 13½	8″·5	22 54·4
	25th	9 13 7	N.16 22	6″·7	22 52·9
Venus ...	1st	9 10 10	N.17 41	9″·6	0 28·2
	9th	9 49 22	N.14 39½	9″·8	0 35·9
	17th	10 27 25	N.11 13½	9″·8	0 42·3
	25th	11 4 30	N. 7 27½	9″·9	0 47·9
Jupiter ...	1st	1 14 7	N. 6 17½	41″·1	16 29·5
	9th	1 14 25	N. 6 17	41″·9	15 58·4
	17th	1 13 55	N. 6 11½	43″·1	15 26·4
	25th	1 12 39	N. 6 2	44″·1	14 45·4
Saturn ...	1st	1 51 7	N. 8 42	16″·6	17 6·4
	9th	1 51 24	N. 8 41	16″·8	16 35·2
	17th	1 51 14	N. 8 38	17″·0	16 3·6
	25th	1 50 41	N. 8 33	17″·2	15 31·6

Mercury is badly situated for observation at the beginning of the month. On the 5th he rises twenty minutes before the sun, the interval rapidly increasing.

Venus is too near the sun to be well observed.

Jupiter rises about two hours after sunset at the beginning of the month, the interval decreasing.

Saturn rises about two hours and a half after sunset, the interval decreasing.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1879.

Squires, H.

To March, 1880.

Barneby, T.
Campbell, Sir A.

To June, 1880.

Lawton, W.
Moss, B.
Slugg, T. S.

To Sept., 1880.

Gray, J. P.
Walter, Rev. H. S.

To Oct., 1880.

Taplin, W. G.

To Dec., 1880.

Andrews, W.
Banks, Mrs.
Bennett, F. J. W.
Dunsken, J.
Rivas, Miss

To Jan., 1881.

Daw, E.

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The Astronomical Register.

No. 213. SEPTEMBER. 1880.

HYGINUS N.

By E. Neison.

(Continued from Vol. XVII., page 259, November, 1879.)

The three perfectly accordant observations of Hyginus N, by Dr. Klein, Mr. Green, and myself, on the night of April 28, 1879, were sufficient to completely establish the existence of this remarkable formation. There could no longer be any doubt. It was certain that there was now a conspicuous black crater-like object in the open plain to the north of Hyginus. Dr. Klein's original statement had been completely confirmed.

Let us consider what this meant. This region had been repeatedly observed by a number of experienced observers, so that we have drawings and maps by Schröter, Gruithuisen, Lohrmann, Schwabe, Kinau, Mädler, Schmidt, and Neison. The region had been repeatedly observed with the express purpose of detecting any such formation as this Hyginus N, if one existed. Yet all agreed that there was no such formation existing prior to 1877. My own observations had been numerous, and under all conditions of illumination, yet not a trace of such a formation was visible between 1870 and 1876. It was certain that so conspicuous an object could not be overlooked and yet much minuter detail be seen all round it and drawn, as was the case with all these observers. It was certain that when these minuter details were seen this great object could not have existed and yet escaped detection. Fortunately this region is one where the influence of libration on the appearance of such an object as Hyginus N is absolutely *nil*. This is a very important fact, a fact which must not be forgotten for a moment. It is absolutely certain that the effect of change of libration on a crater-like depression in the position of Hyginus N must be quite insensible. Be it remembered that the effect of libration on such an object can be calculated with mathematical accuracy, and by the simplest mathematical considerations; no room for doubt exists here. Consequently any change in the appearance of Hyginus N can only come from changes in illumination, so that Hyginus N must once every lunation take every aspect it can present. It would only be necessary to watch the formation continuously for sixteen consecutive days from sunrise, to see the formation under every possible aspect. If, then, there had existed in this region prior to 1876 a crater-like object such as Hyginus N, it seems certain it must have been observed and its existence recorded. But, now, it is equally certain that in this very region there does exist a great black crater-like formation, far too conspicuous to be overlooked. What conclusion are we forced to by the consideration of these two facts? Obviously to the conclusion that during the years 1876—1877 some change must have occurred on this portion of the lunar surface.

But stay, it has been remarked, is there not still another alternative? We will not admit that any change can have occurred on the lunar surface, if it is possible in any other way to account for the observations, and it does occur to us that there may be another manner of reconciling these observations without admitting the existence of any change on the lunar surface. Suppose that this crater-like spot, Hyginus N, is one of those exceptional formations which only remain visible for a few hours, being conspicuous merely under one transient condition of illumination, then rapidly fading away out of sight. If this be so, it is quite possible that none of the observers who examined this region prior to 1876 observed at the right moment, whereas they did after 1877.

Let us consider this view of the question, so as to see how far this hypothesis is tenable. The smaller detail in this region can only be observed within 48 hours of sunrise, and as nearly all the drawings and maps show this smaller detail, they must have been made within this period of 48 hours. When an astronomer examines this region after sunrise, he is equally likely to observe at any period within these 48 hours, so that if he observes it any number of times it is most unlikely that he should not have observed it during all portions of these 48 hours. Now, Hyginus N is only visible as a conspicuous object during these very 48 hours, and from the all-important observations made on April 28th, 1879, we know that it remained visible as a conspicuous object during the eighth to the fourteenth hour after sunrise. This hypothesis we are considering reduces us, therefore, to the assumption that during the entire period prior to 1876 no astronomer happened to observe this region between eight and fourteen hours after sunrise. This is most improbable, nay more, it is certainly erroneous, for there exist drawings by Gruithuisen, Schwabe, and myself during this very period. The whole hypothesis falls to the ground therefore. To utterly destroy it, however, it would be merely necessary to show that so far from Hyginus N being one of those exceptional formations which rapidly lose their conspicuous appearance, it is one of those formations which remain distinctly visible for 24 hours after sunrise, so that it must have been distinct at every observation made near the terminator. If this be proved to be the case, then this hypothesis fails completely, and we must admit that a real change occurred on the lunar surface at some period during 1876—1877. The observations of Hyginus N after this date took, therefore, another direction. They were no longer directed to establishing the existence of N, but towards ascertaining under what conditions and for how long it remained a conspicuous formation. It is only, therefore, observations of this nature that we need consider.

The weather, after giving us an opportunity of settling one point about this interesting formation, Hyginus N, determined to tantalize us by foiling all further observations. The only one of any importance which has reached me, is one made by Dr. von Bienczewski, of Jaslo. It is as follows:

May 28, 1879. Von Bienczewski. Terminator —3° to —5°. Hyginus N seen as a blackish depression, about two-thirds the diameter of Hyginus, and surrounded by a lighter ill-defined border. From its striking dark colour and isolated position, Dr. von Bienczewski thought it could not easily have been overlooked, had it been as distinct prior to 1877.

Though on the *qui vive* for the whole summer I failed to obtain any observation of this formation. My previous experiences had taught me a lesson, and every suitable evening, wet or dry, found me prepared for observation. It was not the slightest use. Fair morning with a blue sky and blazing sun, or dull morning with a downpour of rain, or thunderstorm, or gale, or fog, be the day what it might, the only certain thing was a densely overcast sky in the evening. There was a sort of grim satisfaction in doing the weather. As chance seemed to have it, all the pleasantest events of the year appeared to fix themselves for these particular nights, and temptation after temptation came pouring in to throw up Hyginus N in favour of something of a far pleasanter nature than staring at rain-clouds. But I knew my old enemy the weather was only waiting his opportunity, and that on the first occasion I had yielded, it would have been a magnificent night.

June came, and it was only with the greatest difficulty that I managed to arrange affairs so that I could observe on the only important night, June 26. Thin broken clouds put a stop to everything, every moment promising to break up, but never doing so, though when overhead great gaps could be seen. July came next, but it was a bad month for Hyginus N, as both on the 10th and 25th Hyginus N would be too near the terminator. The 26th was the only night of any value, and I was quite prepared to observe. A magnificent study of rain-clouds rewarded my patience. On the 25th, however, Herr Simon Bauffay, of Fünfkirchen, obtained an observation, but the illumination was still earlier than it had been on March 29th, and Hyginus N had not come into view. Herr Bauffay's observation is in complete accord with those of Mr. Green and myself on March 29, and is given in the *Selenographical Journal* for June, 1879.

August 8th was the first night in the year which gave me a good chance of seeing Hyginus N under favourable evening illumination. The evening promised well, being clear overhead, though misty towards the horizon. As I jumped into my train I thought, now at last I have you. I little knew what was in store for me. My train was delayed, and reached the station some time behind-hand. I noticed that the lamps were not alight at my end of the long platform, it being quite dark, and a thin ground fog lay above the surface. I jumped out anxious not to delay, when my foot caught in something, and I came an awful crash against a post, or something of the kind. I afterwards learned that a coil of rope had been left upon the platform, which was undergoing repair, and that the train should have stopped further up. They picked me up, but my observations for that night were put a stop to. To have looked through a telescope would have been out of the question. All night long I experienced the sensation of some one trying to compress me into a smaller space by means of a sledge hammer. Fortunately the weather for once was merciful, it was not a fine night, clouds rising with the moon and preventing all observations. Next week I felt better and was able to move about, though with a peculiar agonizing dull pain about the heart and soreness in the foot, which warned me I had not seen the end of it. I particularly wanted to see the moon on August 24th, as I knew it would probably be my last chance of seeing Hyginus N at sunrise until the next year. On the 22nd, however, I could hardly move, and until the end of the month all observations were completely at an end, and on the 24th all I could do was to lie back in my chair and thunder the direst imprecations against the weather and all its tricks, as the moon softly sank to rest in the south-west horizon. Had there been an astronomical telescope conveniently placed I would have had myself carried to it, but the nearest was eight miles away—far too long a journey.

On August 8th Herr Báuffay, of Fünfkirchen, observed this region and made a sketch. Hyginus N was distinctly visible as a dark spot. M. Gaudibert was unable to see any dark spot, but saw a whitish spot in this neighbourhood.

On September 7th I again was able to observe, but, as I feared would be the case, though the evening was calm, mockingly calm and beautiful, Hyginus N was completely lost in the shadow of the western arm of the Schneckenberg, Dr. Fergusson, of Cheltenham, thought he had seen Hyginus N, but he had mistaken the crater-like formation within the Schneckenberg for N. No English astronomer seems to have observed Hyginus N between May and October.

October 6th was the next occasion when Hyginus N would be well placed for observation, and due notice was given to all the members of the Selenographical Society, so that they might be ready to seize the opportunity of obtaining a good drawing of Hyginus N under evening illumination. For myself, I determined to avail myself of the facilities so kindly placed at my disposal by Mr. Campbell, and make my observations with the equatorial at the Arkley Observatory. This observatory is remarkably well placed for observing the moon, commanding a very fine horizon. It is in fact the finest placed observatory in England for observing the autumn moon as it rises above the north-east horizon, the moon rising over distant trees in Trent Park, five miles away, and I have seen it in beautifully steady definition when only two or three degrees above the horizon.

The 6th turned out one of those uncertain days with clouds and sunshine at intervals, but I was not very sanguine, and when it clouded over towards seven, it was little more than I anticipated. By ten o'clock, however, after dinner was over, it had once more become fine, though clouds still hung about the moon. When at 11 p.m. we adjourned to the observatory, it was still most uncertain whether any observations could be made. All overhead was quite clear, but round the moon clouds seemed to persistently linger. Gradually the moon got the best of it, and mounting over the clouds, shone out brightly. From 11h. 10m. to 13h. 50m., local mean time, I was engaged in carefully drawing this region. The definition was not of the finest, being unsteady, and thin clouds persistently floating over the moon.

October 6th. Neison. Terminator $+12^{\circ}\cdot5$ to $+11^{\circ}\cdot0$. Hyginus N was visible as a greyish spot, with a dusky interior, having a slight shading. On the south-east is a low, bright peak, and on the south-west a short projection. It lies on the eastern slope of a slight ridge, which forms the western border of a valley. North of N this ridge casts a well-marked shadow. The spot N was sufficiently distinct to at once arrest one's attention, but had more the appearance of a depression than of a crater.

Subsequently I received drawings from other observers, whose results I here append.

October 6th. Durrad. Terminator $+12^{\circ}\cdot2$ to $+12^{\circ}\cdot1$. "Though not at the time well acquainted with the exact locality of Klein's crater, I made a careful sketch and on comparison with the drawings given in No. 16, Vol. II. of the *Selenographical Journal* find a crater-like spot, which I describe as 'lightish grey, not unlike Linne under low illumination, but having more interior shading,' agreeing with the dark spot there given as Hyginus N. A light streak connecting this spot (doubtless Hyginus N) with Hyginus, corresponds with a dark one shown in the drawings before mentioned, and suggests a slope rising westwards; this streak was continued past Hyginus N to the ridge on the north. The central part of the spiral mountain has a fair amount of this shadow, but did not suggest the large crater shown by Neison. A little dent in the spiral mountains nearest to Hyginus N was noticed. The large and roughly oval patch of dark broken ground, north of the spiral mountain, was more or less bounded by the low ridges and mounds in the neighbourhood."

Mr. Durrad's drawing was made on rather a foggy evening with one of Browning's 8¼-inch reflectors. The drawing agrees fairly well with that made by myself, but shows far less detail. Hyginus N is drawn by him as far lighter in colour than it appeared to me, but this may have resulted from the foggy weather, which intensifies differences in tint. His remark about the crater within the spiral mountain is quite just. There is no crater here, only a rounded valley, which looks like a crater at sunrise, the time when I drew it. I knew, of course, that it was no crater.

October 6th. Gaudibert. Terminator $+10^{\circ}\cdot5$ to $+10^{\circ}\cdot0$. Hyginus N seen as a greyish spot, with a small crater on its south.

October 6th. Backhouse. Terminator +12°·2 to +11°·2. On the 6th inst. I observed the neighbourhood of Klein's crater, from 11h. 45m. to 13h. 35m. I enclose a sketch of the chief features at 13h. in its immediate vicinity, and have also marked more distant objects to show the positions. What I took to be N, judging from Neison's sketches in the *Selenographical Journal* a few months back, became more and more conspicuous, but had no resemblance to a crater. I could not even say it was a hollow, for it looked much more like one of the shadows of the spiral mountain. It was much less conspicuous than the central shadow of that mountain, but more so than any other shadow between it and Hyginus. It was irregular and much elongated, and only part was black."

Without the drawing it would have been difficult to make out Mr. Backhouse's description, but on the drawing N is plainly visible, and the northern black portion of Mr. Backhouse's object is clearly seen to be the shadow of the ridge to the north of N. Mr. Backhouse's drawing is fairly in accord with my own, though he shows only a tenth of the detail, and evidently was observing under bad atmospheric conditions.

The observations thus obtained on the night of October 6th threw some light on the constitution of this crater, though being made under evening illumination they were not so useful for determining if it were really a new formation or not. That could only be settled by observations made at sunrise, as the real point to be determined was whether the crater-like object was so distinct at sunrise that it could not have been overlooked.

During November no observations seem to have been made, but the crater was badly placed. On December 4th a favourable opportunity occurred, but bad weather again baffled all my attempts. December 20th was a more promising opportunity, but remained cloudy until after the moon had set. I received, however, two drawings, one from M. Stuyraert, of the Brussels Observatory, and one from Capt. Noble.

December, 20th, 1879. Stuyraert. Terminator +3°·0 to +2°·5. Hyginus N visible as a dark, ill-defined marking, quite as distinct as any other object in this region.

December 20th, 1879. Noble. Terminator +3°5 to +3°·0. "I see what I believe to be Klein's crater N, at the left hand of the base of a nearly isosceles triangle, whereof Hyginus itself forms the apex and the Schnec-

> kenberg and Klein's N the extremities of the base. It appears as a dark, nearly circular depression, in the dark plain north-west of Hyginus."

These two observations conclude the results for the eventful year of 1879. The results will be considered later on. The year had sufficed for one thing. It rendered certain the existence of Hyginus N, and I think I may add that the observations made during the year were such as to render it probable to most selenographers that at last a real case of physical change in the surface of the moon has been practically proved. It only remained to render the proof so conclusive that it would be admitted by the general body of astronomers, who, little accustomed to the study of the surface of the moon, do not readily see the real weight of the evidence in support of this conclusion.

(*To be continued*)

ASTRONOMY IN ITALY.

(Continued from page 183.*)*

Bologna.—The tower of the observatory in the palace of the university (the oldest in Italy) was finished at the end of 1725, and Manfredi, who was charged with the direction of the observatory, placed there some instruments of his own. He was born at Bologna, 1674, and established on the top of his father's house a little observatory, where, with his sisters Teresa and Madalena, and some friends, he amused himself with watching the course of the heavenly bodies. The *Astronomie pour les dames,* which he printed about 1703, described many of their observations, amongst which may be mentioned the study of the great solar spot of 1703, and the discovery of the comet in Capricorn, 1707. Manfredi was also the author of the well-known Ephemerides of Bologna, which were calculated for all the years between 1715 and 1750, and in which his two sisters aided him; thus, as M. Rayet observes, anticipating Mme. Lepaute and Caroline Herschel. Besides ephemerides of the sun and planets, the first volume contains a treatise on astronomical instruments, and a map showing the path of the moon's shadow in the total eclipse of the sun May 3, 1715. Manfredi also observed the conjunction of Venus and Regulus, July 3, 1723, and the transit of Mercury, Nov. 3, 1723. The determination of the latitude of the observatory, in connection with a question then much debated, the invariability of position of the earth's axis of rotation, was one of the first things which he took in hand. With the assistance of the Zanottis he commenced observations of the pole star with three instruments, two movable 3-feet quadrants, and a mural semi-circle. These gave a concordant result of 44° 29′ 52″ as the lat. The following years were devoted by him and his assistants to the observations of planets and eclipses, and especially to numerous observations of the principal stars, with the object of verifying the researches of Bradley on the annual aberration. These were never published, but an iron telescope of about 3 feet focal length made in Bologna, which was employed for some of them, is still preserved as a relic. These labours were interrupted by the death of Manfredi, Feb. 15th, 1739. He was succeeded in the direction of the observatory by his pupil and friend, E. Zanotti, who had been made professor of mathematics at the university. In May of that year he discovered a comet, which he followed till the end of August, and for which he computed a parabolic orbit by a trigonometrical method of his own, which he afterwards applied to the comet of 1742. In February, 1741, the observatory was enriched with several new instruments made in England by Sisson, by the orders and with funds given by Pope Clement XII., at the request of Manfredi. One was a mural quadrant of 4 feet rad., very accurately divided, and furnished with all the means of adjustment used by English artists at that epoch. Observations of Polaris with this instrument gave for the lat. of the observatory 44° 29′ 54″, a result differing little from that obtained by Manfredi in 1728. Another was a meridian instrument of brass, about 3½ feet focus. It was placed near the mural quadrant, so that the two instruments were subsidiary to each other. Zanotti describes in an introduction to the Bologna Ephemerides the infinite pains he took in the adjustment and study of these two new instruments, the perusal of which evinces him to have been one of the ablest astronomers of his time, and well deserving of the eulogies of his contemporaries. With these instruments Zanotti and his assistants, Brunelli and Matteucci, began a continuous series of observations of the sun and planets, and of zodiacal stars. In two years they made a catalogue of 447 stars, which forms a valuable appendix to the ephemerides. Zanotti also continued the Ephemerides for the years 1751 to 1786, and gave them increased precision. He was selected to make observations of the moon to correspond with those of La Caille at the Cape, and his observations were classed with those of Wargentin, Lalande, and Bradley. The meridian line in the church of San Petronio required repairing, as the pavement had sunk. This was done by Zanotti in 1776, and he made use of that immense gnomon for numerous solstitial observations of the sun, which gave him for the latitude of the gnomon 44° 29′ 38″·3, which was the precise result of all the ancient determinations from 1695. He was therefore able to conclude that at least since 80 years the latitude of Bologna had undergone no change. Zanotti died May 15th, 1782. He had for some years been too ill to work himself, and the duties of the observatory, and the conducting of the Ephemerides, were performed by his assistant, Matteucci, who published the volumes for the years between 1787 and 1810. During his time the observatory acquired a fine equatorial by Dollond, which was purchased by the senate in 1780 for 1,000 crowns, and is still in use. After the death of Matteucci, 8th Dec., 1800, there was a blank period in the astronomy of Bologna, till the appointment of the Chevalier Ciccolini about 1803, who published the Ephemerides for the years between 1811 and 1817. P. Caturegli succeeded him as prof. of astronomy and director of the observatory, Oct. 30, 1815, and by him the last volumes of Ephemerides for the years between 1817 and 1836 were calculated. The publication was then discontinued, as altered circumstances rendered it useless. Caturegli died April 28th, 1833. His assistants, Bertelli and Ceschi, successively followed, but the latter soon died, and the observatory had for some years quite lost its former renown. The university could supply no practical astronomer, and the papal government had to send one from Rome. This was I. Calandrelli, nephew of G. Calandrelli, who had long observed at the Collegio Romano, with Conte and Richebach. Under him the observatory resumed its activity, and he procured the substitution in 1846 of a meridian circle by Ertel for the old instruments which he found to be of insufficient exactness. This circle has an objective of 3¾ in. ap., and 4½ feet focal length. The circles are 3·3 feet in diam., and read by 4 verniers to some tenths of a second. Calandrelli, however, was recalled to Rome by Pius IX., in 1848, and M. Respighi, one of his most distinguished pupils, whom he had instructed in all the refinements of observations, was left by him in charge of the observatory. He found in the circle only a very small amount of flexions, and of excentricity, and by observations of Polaris and α Cygni he found for the lat. $44^\circ\ 29'\ 54''\!\cdot\!90$. He also discussed the meteorological and magnetic observations of his predecessors, and observed the principal comets. Called to Rome about 1861 by Calandrelli, he was succeeded by J. Michez, one of the pupils of Santini at Padua, who does not appear to have made many observations, and who died in March 1874. The present director is A. Palagi, who seems to have been compelled by the state of the instruments to renounce almost entirely astronomical observations, so that, as M. Rayet observes, the establishment is now but a sort of museum where some apparatus of historic interest is abandoned to dust and rust.

MODENA.—In Modena, or its neighbourhood, were born Amici, P. Secchi, and M. Tacchini; but a kind of fatality seems to have been connected with its observatory, which never attained the fame of the neighbouring establishments at Bologna and Padua. After Cassini I. various series of observations had been made at Modena by Malvatia (1662), Fontana (1718), and De Zach (1812), to ascertain the geographical position of the tower of La Ghirlandina, which was an important point in the triangulation of the north of Italy; and Bianchi, assistant at the observatory of Milan, made some similar observations in 1815. These appear to have led the reigning Duke, François III., to found an observatory on the eastern tower of the ducal palace, and Bianchi, who had been nominated astronomical professor at the university, received instructions to order some instruments. After long delay he placed in position in 1827 a meridian circle by Reichenbach, 4-in ap. and 5 feet focal length, with circles 3 feet in diam., and an equatorial by Amici, 2½-in. ap., with a double image micrometer of Amici's invention. With these instruments Bianchi from 1829 observed planets and comets, and also altitudes of stars in order to obtain the numerical coefficients of the formula of refractions. He soon, however, discovered the inadequacy of his means for researches of importance, and thereupon relaxed his activity, and confined himself to the preparation of some memoirs on various parts of theoretical astronomy. Having induced in 1858 the Marquis of Montecuccoli to found an observatory in his residence at Modena, he gave all his pains to it, entirely forsaking the official observatory, which in September, 1859, was entrusted to M. Tacchini. Bianchi died Dec. 1868. and Tacchini only remained a short time in Modena, and was himself replaced in October, 1863, by M. Ragona, the present director. The poverty of appliances, which had frustrated the efforts of Bianchi and Tacchini, operated similarly with Ragona, who was obliged to abandon astronomy and bestow all his labours on the study of meteorology and terrestrial magnetism, in which departments considerable progress has been made in recent years, which afford hope of valuable results.

PADUA.—The University of Padua, founded 1264, counts amongst its distinguished professors Galileo, who taught there for eighteen years—1592 to 1610. The observatory was founded by the senate of Venice, in 1761, at the request of Toaldo, professor of astronomy at the university of Padua. It was established in a tower built by Ezzelino III., as a fortress and state prison in 1242. The work of alteration was completed in 1774, and cost 12,000 sequins (£5,600), defrayed by the generosity of the Republic. Toaldo and his nephew, Chiminello, commenced in 1769 a series of barometrical and thermometrical observations, which has been continued without interruption to the present day. Toaldo published in 1774 a treatise on the sphere, a translation of the astronomical tables of Lalande, and a treatise on trigonometry. In 1776 he constructed, with his assistant, in the great hall of the observatory, a gnomon 10½ feet high, with which he fixed the lat. of his observatory 45° 23′ 40″·0. They also made observations with a movable quadrant by Adams, and two telescopes 8 and 10 feet in length. In 1779 a mural quadrant by Ramsden of 8 feet rad. arrived, on which seconds of arc could be read off, and estimations made to 10ths of seconds. The telescope is 3 in. ap. This instrument is still in perfect condition. The lat. obtained with it was 45° 23′ 43″·4. Toaldo died in 1797. Chiminello, who succeeded him, made various observations, and observed the comet of August, 1797, but the war in Italy put a stop to further labours. Chiminello died in 1815. Without accomplishing great things, he had kept the instruments of the observatory in good repair. He was succeeded by G. Santini, who had come from the Milan observatory in 1806. The geographical position of the observatory was still unsettled. Zach, with a repeating circle of Reichenbach, made the lat. in 1807, 45° 24′ 1″·61, which was 21″·6 in excess of that hitherto adopted. Santini, in default of superior instruments, employed a method devised by Gauss, depending upon the time when three stars of known positions attain the same altitude. In this way he found for the lat. 45° 24′ 2″·16. For the longitude he continued the observations of occultations begun in 1805 by Chiminello, and obtained as the long. from Paris 0h. 38m. 9s·1. In 1810, a meridian instrument by Reichenbach, 4 in. ap. served for a series of observations of the planets, and in 1815, a 12-in. repeating circle by the same artist, enabled Santini, by a large number of observations of the sun and stars, to determine the lat. 45° 24′ 2″·50. With an achromatic telescope and micrometer, both by Amici, he made in 1833 a new determination of the mass of Jupiter. The transit circle performs the work both of the mural quadrant and the transit instrument without the defects inseparable from the former; and with a transit circle by Starke, which Santini obtained in 1837, he made and published successively three catalogues of stars of the sixth and seventh magns., 6,621 in all, between declin. + 10° and — 12°·30′. The circle with which these valuable catalogues were made by Santini and his able assistant, Trettenero was about 34 in. diam., reading to 2″ by four verniers. The telescope is 4·3 in. ap. and 5 feet focal length. This work was extended in 1862 by the publication of 2,246 stars between — 12°·30′ and

—15°, the greater number of which were observed by Trettenero, who laboured in the observatory for fifteen years till his death in 1863. In 1858 some important modifications were made in Starke's meridian circle, and with the instrument thus improved Trettenero made the observations which served as the basis of the last catalogue commenced by Santini. The reduction of the observations was made by M. Lorenzoni. At the same time an equatorial by Merz, 3·8 in. ap. on the German principle, with graduated circles and clockwork motion, was installed on the summit of the great tower of the observatory. With this instrument observations of planets and comets were made by Trettenero, and afterwards by Michez and Lorenzoni, who is now director of the observatory. Santini died June 26, 1877, at the age of 91, and in him, as Mr. Dunkin remarks in the memoir of him which appeared in *The Observatory*, vol. i. p. 113: "Astronomy lost one of its most zealous observers during the greater part of this century."

M. Lorenzoni has at present for the principal object of his researches the protuberances and physical nature of the sun. He observed the total eclipse of the sun Dec. 22, 1870, in Sicily, with a direct-vision spectroscope, by Hofmann, and was able to see in the spectrum of the protuberances twenty bright lines, and to measure exactly the positions of some of them. The observatory is now united with the association of the Italian Spectroscopists, in the memoirs of which since 1872 are found numerous contours of the sun drawn at Padua, besides other special memoirs on the same subject. M. Lorenzoni has also been occupied with comets, and in particular with that of Coggia. He has also taken part in the geodesic operations in Italy for the measurement of a European degree. Under his care, and that of his assistant, Professor Albeti, the observatory of Padua has maintained its past reputation.

Rome.—Astronomical observations had been made at the Collegio Romano from a remote time, but the principal observatory, which continued to be used till 1848—a square tower at the east façade of the college—was founded by G. Calandrelli in 1787. When at last he obtained the disposal of the zenith sector, which had been used by Boscovitch in his geodesic operations, Calandrelli made with it a series of observations of 32 zenith stars, from 1801 to 1803, which gave him a first determination of the latitude. A visit paid to the College by Pius VII. to observe the great solar eclipse of February 11, 1804, which attracted the attention of all Europe, led the Pope to grant means to provide for the greater efficiency of the observatory. Among the assistants introduced by Calandrelli were A. Conti and

G. Richebach, both of whom distinguished themselves by their zeal in numerous and varied observations. Political changes forced all three to retire from the College in 1824 (Calandrelli died Dec. 25th, 1827, Conti in 1840, and Richebach in 1841). The P. Dumouchel was then appointed director, and a set of new instruments, in particular an equatorial by Cauchoix, having been obtained Dumouchel, De Vico, and their colleagues commenced their observations. De Vico became director in 1838. Since the observations of his predecessors great improvements had been made in instruments, and the declination of stars more accurately determined. By 4005 observations of stars with a theodolite by Gambey, circles one foot in diameter, and using Bessel's tables of refraction, De Vico concluded the latitude 41° 51′ 52″13, 2 seconds less than his predecessors had made it. He alsc determined the difference of longitude between Rome, Naples, and other cities by observations of eclipses, occultations, and shooting stars. From 1837 to 1842 he observed the satellites of Uranus with the equatorial of 6¼ in. ap. by Cauchoix, by hiding the planet with a suitable diaphragm; and in this way he also observed the division in the outer portion of Saturn's ring. He also observed spots in Venus, determined the period of its rotation, and with the help of the P. Sestini, a skilful draughtsman, made a drawing of its surface. De Vico was likewise associated with the P. Vittorio della Rovere in a minute study of the nebulæ of Andromeda, Orion, the Great Bear, and Hercules. Some drawings of these appeared from 1838 to 1843, which make us regret that these researches were never completed, and that no higher power than 64 was employed. In Sept., 1852, a meridian circle by Ertel was obtained, with which a series of zones of stars as far as 12th magn. was commenced, and indeed carried very far, but the greater part of the work was lost by an accident. By De Vico and his coadjutor, the P. B. Gambara, who was constantly watching with the comet-seeker, and had a special acquaintance with the heavens, 8 comets were discovered, the last of which (3 Oct., 1847) was carefully studied. It was remarkable for its brightness and the development of its chevelure.

The political events of 1848 again suspended the labours of the observatory, the P. De Vico had to quit Rome, and died soon after (Nov. 15) in London; the P. Sestini went to Cambridge, U.S. In 1849, however, when the Jesuit Fathers returned to the College, the observatory recommenced its activity under the direction of the P. Secchi; but he found its locality was altogether inconvenient, and the instruments needed some repairs. Since by the liberality of an ardent amateur, the P. Paola Rosa de

Conti, a powerful equatorial was acquired, it was determined at length to revert to the ancient project of Boscovitch, and to construct halls for observations on the lateral walls and pillars of the dome of Saint Ignazio, and a deficiency in the funds required was supplied by the generosity of Pius IX., and two of the fathers. Leaving the fundamental observations of the science to the great national observatories, rich in personnel and appliances, the distinguished director contemplated the dedication of the Observatory to physical astronomy, embracing planets, double stars, comets, nebulæ, etc. The meridian circle of Ertel sufficed amply for finding the time, and the observation of stars of comparison. The telescope of Cauchoix, equatorially mounted, would be suitably employed for comets and sun spots; whilst for more delicate physical researches, double stars, etc., a 9-in. telescope by Merz was found adequate. All was completed in October, 1853, and in that month the Observatory was formally inaugurated by Pius IX.

In the meridional room is placed the meridian circle by Ertel, between two pillars of granite; the telescope is 3¼ in. ap., the circles 27 in. diam., reading by four microscopes. It has two collimators, north and south, and near it are two clocks; a sidereal with mercurial compensation by Dent, and another showing mean time, which gives a cannon of the Fort St. Angelo the signal of mean noon. The equatorial room has a cylindrical cupola of wood 25 feet in diam. The telescope is 9 in. clear ap., and 14 feet focal length; the circles are 15½ in. diam., and read, the one to a second of time, and the other to four seconds of arc. The mounting is very stable, and flexions are obviated by the plan of Merz. Another small equatorial serves for the observation of the solar spots by projection. P. Secchi published since 1856 researches on double stars; and in 1866 and 1875 two other memoirs on the same subject. The excellence of the equatorial of Merz, and the transparency of the Roman sky, enabled him to scrutinise successfully the well-known nebulæ in Lyra, Andromeda, and Hydra; of the last two he made drawings, and he verified the delineation of the nebula in Orion, given by Herschel. These and other observations, with the discussion and publication of the meteorological and magnetical observations begun by Conti and Richebach, engaged the astronomers of the Collegio Romano till about 1860. They had also, in 1854—55, been obliged to remeasure in the Appian Way the old base line of Boscovitch, on which rested the triangulation of central Italy. The reading of the memoir on stellar spectroscopy, by Donati, induced P. Secchi to obtain from Hoffmann in Paris a direct vision spectroscope, similar to the one with which Donati worked. With this he drew in 1863 the spectra of Sirius, Betelgeux, and Antares; and in order to observe the spectra of fainter stars he devised himself a spectroscope with which he could deal successfully with some as low, occasionally, as 7th magn. His studies on a large number of stars were set forth in a series of memoirs from 1867 to 1872, and led him to the classification of stars into four types. He was able, also, to measure the position of certain lines, in particular of F, D, and C, and to satisfy himself that the stars, like the sun, contained many of our elementary substances. In some instances he ascertained, at the same time as Huggins, the change of refrangibility of their rays by reason of the star's proper motion in space. His experiments made on the sun in 1842, 1852, and 1855, with a thermo-electric pile had proved the decrease of the heat from the centre of the disc to the limb, where it was only half the amount of the centre; a diminution evidently due to an atmosphere above the photosphere of considerable absorbing power.

P. Secchi observed the total eclipse of the sun July 18, 1860, in Spain, and deduced the true explanation of the protuberances, as was done by other observers. Availing himself subsequently of the discovery of MM. Janssen and Lockyer, he zealously pursued the study by a series of continuous drawings and measurements. His well-known book on the Sun, however, renders it unnecessary to dwell upon these here. Another on the stars was completed shortly before his death, which took place February 26th, 1878. A brief biographical notice of him is in *The Observatory*, vol. i. p. 402. Before concluding this account of the Observatory of the Collegio Romano under the old regime, we must mention the memoir of the P. Paolo Rosa, "Studies on the Solar Diameter," Rome, 1873, in which he thinks he has shown that the sun's diameter is subject to variations of short periods. M. Rayet observes, "If we consider that the astronomers are few in number, and often distracted by various professional obligations, we cannot but admire the importance of the results they have obtained, and would wish that all the observatories of Europe brought to astronomy similar benefits."

When the religious corporations were suppressed in 1873 the government, in consideration of the distinguished astronomer, left untouched that part of the College where the Observatory was situated; but on the death of P. Secchi this exception ceased, and in June, 1879, the government called P. Tacchini from Palermo to the directorship of the Observatory; at the same time uniting with it the central office of meteorology and telegraphy, from which periodical notices are issued, and where the observations of nearly a hundred Italian meteorological stations are collected and published. The first volume is to appear in June, 1880. In the same department is included the rainfall service pertaining to the basins of all the Italian rivers; thermographic observations, embracing about a thousand stations; and the time-service organised for 42 provinces. Lastly, there are the meteorological and astronomical observations made in the Roman College. These are all to appear in one volume; the first part embracing metereology, the second meteoric observations at the Italian stations, the third the observations both meteorological and astronomical made at the observatory of the central office. In addition to this a daily bollettino is published of the telegraphic despatches, which at the end of the year will make a volume of 1,500 pages, containing, besides other meteorological matter, the agrarian meteorological notices, which are published at intervals of ten days. For all this the government has afforded sufficient means, which it is proposed to increase in 1881. The personnel, too, has been augmented, and Prof. Millosevitch, who was astronomer at the Nautical College in Venice, was appointed vice-director since June 1879. Besides, there are three assistants and three compilers, and other employés are contemplated in 1881, so that with the new year the institution may be regarded as complete. M. Tacchini, who has kindly communicated these particulars at our request, says that he has, since June, 1879, continued his solar researches, and with M. Millosevitch observes comets and small planets with the Merz refractor. These and other astronomical observations will appear in the volume for 1879. As soon as the meridian circle shall have received some proposed improvements it will again be employed, and as much will be done as possible to do honour to the name of Secchi, the founder of this observatory.

M. Tacchini, moreover, continues to direct the organisation of the observatory on Mount Etna, the project of which emanated from himself. (See *The Astronomical Register*, vol. xvii. p. 250.) The building is finished, and he hopes in the current year to place the instruments in it; amongst which is to be a refractor of 13·8 in. aperture. He has also organised two meteorological stations, one on the Cimone, 6,560 feet above the sea, and the other on the Gran Sasso, about 9,840 feet above the sea; these will complete the meteorological service for high elevations.

Observatory of the Capitol.—This observatory is situated on the top of the eastern tower of the Palace of the Capitol. In 1848 Pius IX. granted funds for the purchase of a meridian circle, and for the building; and Ignatius Calandrelli, who had been coadjutor successively to G. Calandrelli, Conti, and Richebach at the Observatory of the Roman College, was charged with its direction. The circle by Ertel was installed March, 1853: its telescope is 3·7 in ap., and its circles about 2 feet diam., are read by four microscopes, and admit of the estimation of hundredths of a second. With this instrument Calandrelli, by numerous observations on four stars in 1854—55, concluded the latitude of his observatory, 41° 53′ 34″·35. Finding considerable uncertainty in the co-ordinates, of even the largest stars, in the catalogues, he observed certain selected stars with a view to a very precise determination of their positions, and their proper motions, and in particular Sirius and Procyon, the variability of whose proper motions had already been discussed by Bessel and Struve. The catalogue and two memoirs were published in 1855, 1857, and 1863 in the *Atti dell' Accademia dei Nuovi Linai.* Calandrelli also made minute investigations of refractions at a low altitude above the horizon, and calculated the orbits of many minor planets (Egeria, Irene, Parthenope, etc.) and of comets. For the observation of these bodies he was fortunate at last (1860) in obtaining by the generosity of the Marquis de Ferraioli an equatorial by Merz, with a telescope 4⅓ in. ap., driven by clockwork; but he had not long the satisfaction of using this fine instrument, as he was taken seriously ill early in 1865 and died February 12th, 1866.

M. L. Respighi, astronomer at Bologna, appointed his successor, commenced an active career. By a long series of observations in 1868 of a great many stars, symmetrically situated on both sides of the zenith, both directly and by reflection, he obtained as the latitude 41° 53′ 33″·55, which is less by 0″·80 than Calandrelli's, but agrees satisfactorily with that deduced trigonometrically from the Observatory of the Roman College. In 1868 was obtained a zenith telescope from Ertel at Munich, 4·2 in. ap. It is mounted on two marble pillars, like a transit instrument, for which it can be used. Between the pillars is a well nearly 70 feet deep, at the bottom of which is a bath of quicksilver upon copper, so that the amalgamation gives very steady stellar images. This instrument is used by M. Resphigi for measuring the declinations of stars near the zenith of Rome. The results are remarkably accordant. The scintillation of stars investigated by Arago, Montigny, and C. Wolf, and by M. Respighi at Bologna, was studied in a long series of observations by him in 1868 and 1869, and a notable discovery made of the difference as seen in the spectrum, according as the stars are in the west or east. The last and most important of the labours of M. Respighi, begun at the end of 1868, are relative to the spectrum analysis of the protuberances, and the physical constitution of the sun, and they continue to be one of his principal occupations.

Some of his valuable researches and drawings will be found in Proctor's *Sun*. He has sometimes made use of Fraunhofer's plan in 1814, placing a prism in front of the objective of the equatorial. A prism capable of covering the whole surface of the objective, having a refractive angle of 12°, was constructed by Merz, and gave stellar spectra of great brilliancy and remarkable distinctness: and later, when he observed the total eclipse of the sun, Dec. 12th, 1871, in India, he adopted the same plan for the observation of the corona, and found the different spectral images of the protuberances were not absolutely identical, and that the bright line, 1474 of Kirchoff, was peculiar to the spectrum of the corona. Occupied by day in his solar researches, M. Respighi's nights are devoted to the scintillation of stars, and to the meridian observation of stars between the 1 and 6 magn., which are used by the officers of the Italian Etat-major in their geodesic operations.

OBSERVATIONS MADE DURING TOTAL SOLAR ECLIPSES.

(*Continued from page* 191.)

XXV. The entirely altered illumination of the heavens during totality causes the clouds above the observer, which are no longer projected upon a bright background, to appear much nearer than before. If he is looking towards the zenith as totality comes on the observer sees the clouds above him appear to drop towards the earth. . . . The gloom about the observer also alters his estimate of distances. If his station be upon a mountain, the plain below him . . appears to become more distant." . . "The increasing nearness of clouds may be perceived long before the eclipse is total." XXVI. "To an observer standing within the area of totality, the heavens above appear dark, while the horizon is brilliantly illuminated with tints of red and orange. Two theories have been put forward as to the origin of these colours; the one that they are produced in the same manner as the ordinary tints of sunset, and the other that the red clouds are illuminated by the light of the hydrogen chromosphere, while the orange clouds are lit up by the lower incandescent strata that give out the brilliant D line, or even by the mixed spectrum of Young's reversing layer. According to the first of these theories, the tinted clouds would derive their illumination from the area outside the cone of totality, illuminated by the rays from the thin solar crescent. The light which passed through the lower layers of the atmosphere would lose most of the short wave lengths, and we shall have, as in an ordinary sunset, red on the lower clouds and yellow above.

According to the direct illumination theory, the tinted clouds would all be situated just outside the area of *total* darkness. The orange clouds would be in a zone at the outside, and the red clouds would occupy a decidedly broader zone within them. . . . In the red zone the heavens and the clouds, whatever their altitude, would all be tinted red, and in the orange zone all would be orange. On this theory the orange clouds would first make their appearance on the approach of totality, and then the red, and after totality first would come the red and then the orange." The observations show that the tinted clouds are by no means confined to a narrow zone around the area of totality. They have been observed many hundreds of miles away from the shadow path, and apparently by others within the area of total darkness; colours have also been observed either before totality commenced, or after it was well over. . . . In no case are the clouds described as *suddenly* changing colour from red to orange, or *vice versa*. . . The similarity of the tints to those of sunset, or sunrise, seems to have struck most of the observers. XXVII. Arcs of prismatic colours (with one exception) arranged concentrically about the sun, with a radius from one to five degrees, have been seen. In some instances more than one concentric arc has been observed. the order of colours is mentioned only in two cases, where the red end of the spectrum was outermost. Iridescent clouds are described at distances varying from 15° to 50° from the sun's place, and both the rainbow-tinted arcs and iridescent clouds have been observed not only during the period of totality, but also before and after the total phase, as well as at stations situated at a considerable distance from the path of totality. XXVIII. "Before totality, and when the sun is only about five-sixths covered, the colour of the sky in the zenith, and down to a distance of some ten or twenty degrees above the horizon, begins sensibly to change. From the ordinary blue of full daylight it gradually assumes a darker hue, which has been variously described as purple black, violet, azure, and indigo. . . . At the moment of totality the suddenly altered conditions of illumination give rise to a further change of colour. . . . which no doubt depends greatly on the atmospheric conditions at and around the observer's station, and has been variously described as purple, indigo, slate colour, olive, and deep violet like the colour of the spectrum near H." XXIX. Some observers describe the moon as being darker than the surrounding sky. "It should be remembered that any illumination due to our atmosphere would stretch equally over the place of the dark moon, as well as over the area immediately surrounding the corona. The greater darkness of the moon must, therefore, either be due to a sense of contrast (the brighter layers of the corona being on the inside) or to a very widely spread outer corona. . . . At the time of a total eclipse the moon is lit up by full, or nearly full, earthshine, yet none of the observers record that they were able to detect any of the lunar details, though many of them carefully examined the disc for that purpose. . . . The estimates as to the tint of the moon's disc during totality vary greatly:" greyish brown; dark grey; black; intensely black; green; greenish grey; etc. XXX. "It appears that during totality the illumination of the heavens is such that only stars of the first and second magnitude are generally visible; stars of the third magnitude have been recognised, but apparently only with difficulty by those who have previously acquainted themselves with their position." XXXI. "During totality, while the heavens above the observer are comparatively dark, the sky upon the horizon appears brightly illuminated, so that mountains or buildings at a distance seem to stand up with sharply defined outlines against the sky. Thus, while objects at hand are nearly invisible, those upon the horizon are more clearly seen than they were immediately before totality. Möller and McTaggart evidently judged that the light by which they were enabled to distinguish the objects in their immediate neighbourhood was principally derived from the clouds and sky near the horizon, rather than from direct illumination by the corona; and this becomes the more probable when we take into consideration the fact that no shadows are cast during totality. The light from the horizon coming in all azimuths naturally casts no shadow, and it would appear that the illumination derived from this source is so great that the eye cannot distinguish between the brightness of objects so lit up, and those which are also illuminated by the light of the corona and prominences." XXXII. "From the chapter on the brightness of the corona, it will be seen that but little of the light by which objects are rendered visible during totality is derived from the corona and solar appendages. . . . the light derived from the heavens is chiefly due to an illuminated belt of clouds and sky near to the horizon. As might therefore be expected, the degree of darkness during totality differs greatly according to the terrestrial conditions surrounding the observer, the clearness of the atmosphere, the clouds and objects upon the horizon, the size of the area of totality, and the position of the observer within the area of darkness at the moment of his observation." XXXIII. "The red and orange light coming from the bright belt of sky and clouds upon the horizon, gives to objects around the observer a peculiar and unnatural tint, which, together with the darkness above, causes an unearthly effect that has frequently been remarked. . . . Any surface of water from which the light of distant clouds can be reflected becomes, as might be expected, brilliantly lit up with reflected tints." XXXIV. "Small bodies floating at considerable altitudes within the area of totality, would, in addition to the lateral illumination, be lit up on their under sides by light derived from the surface of the earth outside the area of the totality. It seems possible to conceive that such small bodies might thus appear as bright specks upon the dark background of sky. But this theory will not satisfactorily account for all the observations collected in this chapter. Murray, observing during the annular eclipse of 1851, speaks of five stationary lines across the disc of the moon. The observers at Mattoon, in 1869, speak of faint whitish bodies floating past a few moments before, as well as during totality. And Weidler's friend in 1738 and Myer in 1869 describe bright bodies seen long before and long after totality. It should, however, be remembered that there are many well-known instances in which insects, thistle seeds, and even snow flakes have been mistaken for meteors in ordinary sunlight." XXXV. "During the same eclipse very contrary atmospheric conditions have been recorded as existing during the period of totality at different stations." Thus in 1869, nine observers describe the wind as rising, or blowing in gusts as totality came on; while five observed that the wind fell, and totality was ushered in by a comparative calm. Two noticed that the wind shifted during totality; but all concur in describing a change in the atmospheric conditions, which they regard as due to the approach of the totality shadow. Only two failed to notice any change, one in 1860, the other in 1869. XXXVI. "The general results which appear to have been attained, are that the height of the barometer is not materially affected by the passage of the shadow, but that there is a marked depression of temperature as the time of totality approaches, which varies in amount according to local circumstances, the season of the year, and the height of the sun above the horizon at the time of central eclipse. . . . observers frequently mention that they were conscious of a sensation of chilliness at the time of totality." Only actually deposited during the period of central eclipse. XXXVII. seven observers, as far as is known, mention that dew was "There seems to be but little difference of opinion as to the effect produced by an eclipse upon the vegetable kingdom. Flowers and leaves which ordinarily close at night, begin long before totality to show signs of closing up." Ants have been observed going on working during totality, and grasshoppers to have been stilled by the darkness. Higher up in the scale of life the effects seem to vary with the particular temperament of the animals observed. XXXVIII. The evidence in favour of a disturbance in the magnetic elements during eclipses is not strong. "Secchi, Whipple, and Perry state that there is no evidence of disturbance due to the eclipses of 1860, 1870, and 1871, and it should be remembered that Whipple and Perry have had the advantage of being able to compare at their leisure the photographic records of delicate instruments." XXXIX. "The indentations or notches beneath prominences spoken of by so many observers, are, it can hardly be doubted, due to irradiation, which causes the brilliant prominences to seem larger than they really are, and consequently to appear to overlap the limb of the dark moon. . . . As to Ulloa's account, there can be little doubt, now that it is examined by the light of modern observations, that "the small luminous point" seen by him and his companions "near the edge of the moon," was a prominence, the summit of which was slowly uncovered by the western limb. . . . The name "trou d'Ulloa" has come to be recognised as meaning a hole, seen apparently through the disc of the moon during an eclipse. But no such hole, or even spot of light, has . . . . ever been observed. The notches in the moon's limb under the prominences, which are clearly to be seen in all the corona photographs . . . . may be easily explained as due to an enlargement, or spreading of the images of the prominences known as photographic irradiation. A similar spreading is to be found in all over-exposed photographs of luminous objects. XL. "When the amount of atmospheric illumination during totality is remembered, it will probably not be thought remarkable that the zodiacal light should have been looked for without success by Goldschmidt, Rapatel, and Newcomb, during the eclipses of 1860, 1868, and 1869. . . . Intra-Mercurial planets have also been carefully searched for during totality, but without success, by Secchi, Newcomb, Gould, and Schott." XLI. Many observers, some of whom gave their especial attention to the subject, were unable to detect any shadow cast by the corona; on the other hand, three perceived a shadow, but they do not mention any precautions taken to satisfy themselves that the shadows observed were really cast by the corona. "It would . . . . appear that the total illuminating power of the corona is considerably less than that of the full moon, though the prominences and lower parts of the corona—or, at all events, of the corona of 1860—were brighter, area for area, than the full moon. . . . The dazzling brightness of the corona has been remarked upon by Biela, Struve, and Ranyard; but the eye when dazzled would not be able to distinguish between the parts of the corona from which the brilliant light is derived, and the whole corona would thus be spoken of as dazzlingly bright, when, no doubt, the chief source of illumination was from the prominences and coronal area immediately adjacent to the moon's limb."

The chapters from which we have made the above extracts occupy 254 pages; the last three, of considerable length, extend to page 720. XLII. After discussing Polariscopic observations Mr. Ranyard says, "It appears from all the above observations that the atmospheric polarisation is generally sufficiently strong to overpower the radial polarisation of the corona at a short distance—probably not more than 1° from the moon's limb." The light of the prominences (omitting dubious observations) is found to be unpolarised. Diagrams of various polarising instruments used in the observations are given. The observations of fifty-two observers are collected in this chapter. Of these, sixteen were unable to detect any traces of polarisation in the light of the corona, and reasons are assigned why the value of their evidence is considerably diminished. Arago and eleven others found the light derived from the coronal area to be polarised, but did not determine the plane of polarisation. All the observers (except those whose observations do not seem conclusive) who have determined the plane of polarisation of the light derived from any part of the coronal area agree in finding that the light is radially polarised; among these is Mr. Ranyard himself (1870). At the end of this chapter are suggestions for polariscopic observations of future eclipses, by Mr. Ranyard, Prof. Stokes, and Prof. Pickering. XLIII. "The history of the spectroscopic analysis of the light of the solar surroundings" may be said to commence with the observations made during the eclipse of 1868. . . . During the partial phases of a solar eclipse, until the last few seconds before totality no difference is perceptible in the spectrum of the light derived from the solar crescent. . . . As to the spectrum of the lower parts of the chromosphere, Young, observing in 1870 with the slit of an analysing spectroscope placed tangentially to the limb of the sun at the point where the last ray was about to vanish, saw the ordinary solar spectrum gradually fade away; and "all at once, as suddenly as a bursting rocket shoots out its stars, the whole field of view was filled with bright lines." His observation was confirmed by Pye, who was observing at the same station with an integrating spectroscope; and Burton and Nobile, who were observing in Sicily, noted the one about 25, and the other not less than 40 bright lines in the spectrum of the cusps of the thin solar crescent. In 1871 the bright lines at the cusps were seen by both Maclear and Herschel. . . . At the annular eclipse of 1872 Pogson observed the reversion of lines on the formation, and again at the breaking up of the annulus, and in

1874 Stone again confirmed the observation of Young. . . The observers differ greatly as to the length of time during which the bright lines have remained visible. Their estimates vary from one-eighth of a second to as much as seven seconds; intervals of time which would correspond to depths of the reversing layer ranging from about one-sixteenth of a second of arc to as much as three-and-a-half seconds of arc—or, speaking roughly, from about 30 miles to as much as 1500 miles in thickness. . . Nobile noted that the bright lines at the cusps were visible at least five minutes before and after totality, and Maclear that they were visible about "three minutes before middle eclipse." . . Some of the conspicuous Fraunhofer lines have not been seen reversed in the spectrum of the photosphere, and others are but rarely seen reversed. It becomes, therefore, an interesting question to examine what evidence there is to show that all the dark lines of the solar spectrum have been seen reversed at the beginning or end of totality. Young thought that the bright lines seen by him corresponded with the ordinary Fraunhofer lines, and he remarks that though "it would be very rash on the strength of such a glimpse to assert with positiveness that these innumerable lines correspond exactly with the dark lines of the spectrum," yet the general appearance of the spectrum and the "grouping of the lines seemed perfectly familiar" to him. . . . Pogson writes that he "saw all the dark lines reversed and bright." Stone is more careful, and adds, "My impression was that all the Fraunhofer lines were reversed; but this is, of course, only an impression. I can only state as a matter of fact that a very large number of bright lines were seen." It is especially worthy of remark that the line which appeared to Pogson to be the most brilliant of the bright lines seems to have corresponded in position with B, a marked Fraunhofer line which has not been seen by Young as reversed in the spectrum of the chromosphere. The reading of the cross wires of his micrometer "clearly showed that the line was B."

Plate I. (in two parts) exhibits the evidence of observers with regard to each of the lines of the coronal spectrum. Young in 1869 fully satisfied himself that the green line corresponded with, and was the reversal of the Fraunhofer line 1474, and this was confirmed by others, and again by Young himself in 1870, though he has since found that there is not an accurate coincidence. More recently he has proved that the Fraunhofer line 1474 is unmistakably double, and that the more refrangible component matches with the chromosphere line, and the other belongs to the spectrum of iron. Mr. Ranyard thinks that the identity of the corona line with the more refrangible of the two Fraunhofer lines still requires further confirmation. He adds, "Considering the faintness of the corona line, and the great dispersion necessary for such an accurate observation, as well as the fact that a velocity of less than five miles a second of the matter of the chromosphere to or from the observer would account for a similar displacement, there seems no great prospect that the identity of the corona line with the more refrangible of the two 1474 Fraunhofer lines will be established at future eclipses with very much greater certainty than has at present been attained to." The hydrogen lines of the coronal spectrum also seem to be satisfactorily identified. Faint lines less refrangible than 1474 have likewise been observed, of which five woodcuts of spectra present the evidence of as many observers. The various estimates of brightness of the continuous spectrum, Mr. Ranyard observes, "may no doubt easily be reconciled when we consider that the brightness of the corona differs considerably from eclipse to eclipse, and that the brightness of the spectrum as seen with analysing spectroscopes would differ greatly as the slit was placed upon various parts of the corona." Some observers describe the continuous spectrum on which the bright lines of the coronal spectrum have been seen as scarcely coloured; others speak of definite colours. Mr. Ranyard remarks that it "would be well that during future eclipses greater attention should be directed to the distribution and intensity of the light at various parts of the continuous spectrum of the corona;" and he adds in a note, "Such an examination would, no doubt, throw important light on the constitution of the corona." As to the presence of absorption lines, some observers were unable to detect any trace of them in the continuous spectrum of the corona; Janssen, however (1871), observed the dark line D, and some very faint dark lines in the green. Stone (1874) saw dark lines in the spectrum of the outer corona. The continuous spectrum appears to have been traced sometimes to a distance of more than a solar radius from the limb. A diagram shows the distance from the sun's limb to which the bright line 1474 has been traced by twelve observers, some in a clear, others in a hazy sky, or through clouds (the second class, for a reason given, must be received with caution). By Rayet (1868) it was traced nearly 7′; by Stone (1874) 45′, both with clear sky. The distance to which the 1474 line can be traced in various directions from the sun's limb is still uncertain. Young found in 1870 the area in which this line was visible extending rather further east and west than north and south. Respighi (1871) speaks of the green zone as sensibly circular. The enormous extension observed by Stone (1874) was from the western limb. A woodcut exhibits the distance from the sun's limb, to which lines of the hydrogen spectrum have been traced by eight observers. Amongst these are Rayet (1868) nearly 7′; Janssen (1871), and Stone (1874), both 10′. None of the observers who have seen the faint lines less refrangible than 1474 mention the height to which these lines could be traced above the sun's limb.

Does the matter corresponding to the 1474 line form a continuous envelope uniformly incandescent about the sun? Observations by Herschel and Respighi (1871) favour the affirmative, but more observations are wanted. On the brightness of the lines seen in the spectrum of the corona Mr. Ranyard says, "From the fact that so many observers have been able to trace the 1474 line to a considerable distance from the sun's limb, while they have not seen any trace of the lines of the hydrogen spectrum, it will be gathered that the 1474 line must, as observed with analysing spectroscopes, appear considerably brighter than the hydrogen lines in the spectrum of the corona." And again, "Of the observers who have made use of integrating spectroscopes, all who refer to the relative brightness of the lines agree that the 1474 line was decidedly the brightest." With regard to the actual intensity of the bright lines of the coronal spectrum he says, "We have very little evidence to guide us. Maclear, in 1871, was unable to detect any spectrum from any part of the corona with a telescope and spectroscope, with which he had been able on the nights immediately before the eclipse to see three lines in the spectrum of the nebula in the sword-handle of Orion. . . . Whatever was the cause of the loss of light, Maclear's observation shows that the monochromatic light of the corona is not strong compared with that emitted by the nebula in Orion. . . . That the monochromatic light emitted by the corona forms only a small part of the total light is proved by the strong polarisation of the light of the corona, which would certainly be masked if the light emitted by incandescent gas bore a large proportion to the dispersed solar light. But the very small proportion of monochromatic light is still more strongly proved by the fact that so many of the spectroscopic observers have seen the continuous spectrum of the corona, and yet have failed to detect any trace of bright lines." It seems clear that "the monochromatic light emitted by the gaseous matter of the corona is very small compared with the total light of the corona." "An attempt was made during the eclipse of April, 1875, to photograph the spectrum of the corona, but, as might have been expected, the attempt failed; and only images of the prominences and the bright erupted matter of the chromosphere were obtained. The total light of the nebula of Orion has never yet, even with long exposures, left any trace upon photographic plates. We are very far, therefore, from obtaining a photograph of its spectrum. It will be remembered that the light which passes between the jaws of the slit of a spectroscope is not only distributed over the whole length of the spectrum, but each monochromatic image of the slit is considerably weakened by reflections from the surface of the prism or prisms and lenses, as well as by absorption from the glass of which they are composed. Some seconds are required to obtain a photographic record of the brighter parts of the corona, even with short-focused instruments, like those with which the Syracuse and Indian photographs of 1871 were taken; and when we bear in mind the extreme faintness of the spectral images compared with the total light of the corona, it will be seen that there is but little chance of obtaining a record of the lines of the coronal spectrum by means of the photographic processes at present in use." This chapter has many woodcuts of spectra and diagrams, showing the positions of the slit, etc., and a description of the instruments made use of by the different observers in the observation of the spectrum of the corona. At the end are suggestions for spectroscopic observers of future eclipses; I. Suggestions prepared by Prof. Young, in 1871; and, II. Further suggestions by Mr. Ranyard, in 1877. The spectra of prominences which have been seen during total eclipses have not been discussed, since the spectra of the chromosphere can be studied at leisure without an eclipse.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

AURORÆ.

Observers of the August Perseids must have noticed the displays of Auroræ on the nights of August 11, 12 and 13.

During the hour preceding midnight on August 11, there was a strong glow over the horizon in NNW., with numerous streams of pale light ascending amongst the stars of Ursa Major, but the display was not striking at any time. On August 12, at about 10h. 20m., the phenomenon reappeared under a more imposing aspect. An arch of crimson light formed just over the horizon, below the stars β, γ Ursæ, while a large number of vertical rays were interspersed amongst the stars in the region extending from Arcturus to Capella. After some rapid changes the Aurora became fainter, but it remained present during nearly the whole night. At 13h. some streamers were again noticeable, and at 14h. I saw the auroral glow condensed above the NNW. horizon, but after 11h. it was never very conspicuous. On the 13th there were further indications of an Aurora. The sky was much lighter than usual in the N. quadrant, though no streamers were observed, and I was much struck with the exceptional brightness of the night. The moon set before 10h., yet at midnight the air was not dark, objects around could be discerned with remarkable distinctness beneath the luminous gauze of the aurora apparently diffused over the sky.

Ashley Down, Bristol: W. F. DENNING.
1880, August 15.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN SEPTEMBER, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

The month of July has furnished 41 observations of 12 spots on the floor of Plato, by three observers, Mr. Allison, Mr. Gray, and Mr. Williams, the distribution of which is as follows:

Spots	1	3	30	4	22	14	5	17	6	13	19	16	
	1							1					2 Allison.
	3	3		3	1	3		1	1				15 Gray.
	3	3	1	3	2	2	2	2	1	1	2	2	24 Williams.
	7	6	1	6	3	5	2	4	2	1	2	2	41

On comparing this table with that of the distribution in the first half of 1880, *English Mechanic*, July 30, p. 488, it will be seen that spots 25 and 7 have not been observed, but that spot 30 in group I. was seen by Mr. Williams on July 22, 13·10, who recorded it as quite plain, not very much fainter than spot 3. The spots of group I., viz., 1, 3, 4 retain their pre-eminence for visibility, spot No. 1 having been seen seven times, and spots 3 and 4 each six times, visibility = 857. The next in order is spot No. 14 with a visibility of ·714, after which come 17 and 22, visibility 17 = ·571, 22 = 430, and next 5 and 6, visibility ·286, which completes group II. The spots in this group most frequently seen in July were 14, 17, and 22, 14 and 22 having increased in visibility, but 17 has declined. Mr. Williams saw on July 22, 13·10, a light streak extending from 17 to 5 and from 5 to 14. The increase of visibility of 14 is in accordance with increase of activity.

Corinium House, Romford Road,
Stratford, E.: August 20, 1880.

ASTRONOMICAL OCCURRENCES FOR SEPTEMBER, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
Wed	1	0	Conjunction of Uranus and Sun Sidereal Time at Mean Noon 10h. 44m. 6·13s.	1st Ec. D. 1st Oc. R.	11 20 41 14 24	12 12·7
Thur	2		Sun's Meridian Passage 0m. 36·52s. before Mean Noon	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	8 37 9 29 10 52 11 42	12 8·8
Fri	3	9	Conjunction of Moon and Mercury 6° 12′ N.	1st Oc. R. 3rd Ec. D. 3rd Ec. R. 3rd Oc. D. 3rd Oc. R.	8 50 11 17 25 13 47 36 14 48 16 56	12 4·9
Sat	4	4 52	● New Moon			12 0·9
Sun	5	15 17	Conjunction of Moon and Venus 6° 50′ N. Conjunction of Moon and Mars 6° 18′ N.	2nd Sh. I.	16 11	11 57·0
Mon	6					11 53·1
Tues	7	6	Conjunction of Mars and Venus 0° 31′ N.	2nd Ec. D. 2nd Oc. R. 1st Sh. I. 1st Tr. I.	11 11 1 15 15 16 3 16 47	11 49·1
Wed	8			1st Ec. D. 1st Oc. R.	13 15 18 16 10	11 45·2
Thur	9			2nd Sh. E. 2nd Tr. E. 1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	8 11 9 31 10 32 11 14 12 46 13 27	11 41·3
Fri	10			1st Ec. D. 1st Oc. R. 3rd Ec. D.	7 44 1 10 36 15 19 13	11 37·3
Sat	11	6 24	☽ Moon's First Quarter Near approach of θ Ophiuchi (3½) Saturn's Ring: Major axis=44″·00 Minor axis=11″·54	1st Sh. E. 1st Tr. E.	7 15 7 53	11 33·4
Sun	12					11 29·5
Mon	13	7 14 8 5 9 42 10 43	Occultation of 50 Sagittarii (6) Reappearance of ditto Occultation of B.A.C. 6671 (6) Reappearance of dttto			11 25·5
Tues	14	9 39 10 31 13 12	Occultation of B.A.C. 7043 (6½) Reappearance of ditto Occultation of B.A.C. 7097 (6)	3rd Tr. I. 3rd Sh. E. 3rd Tr. E. 2nd Ec. D. 2nd Oc. R.	8 3 8 7 10 11 13 46 8 17 31	11 21·6
Wed	15		Illuminated portion of disc of Venus=0·955 Illuminated portion of disc of Mars=0·995	1st Ec. D.	15 10 2	11 17·7
Thur	16	21	Superior conjunction of Mercury and Sun	1st Sh. I. 2nd Tr. I.	8 7 9 12	11 13·7

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
			(*16th day continued.*)	2nd Sh. E.	10 49	—
				2nd Tr. E.	11 49	
			Sidereal Time at Mean Noon 11h. 43m. 14·43s.	1st Sh. I.	12 26	
				1st Tr. I.	12 58	
				1st Sh. E.	14 40	
				1st Tr. E.	15 11	
Fri	17		Sun's Meridian Passage 5m. 54·8s. after Mean Noon	1st Ec. D.	9 38 47	11 9·8
				1st Oc. R.	12 20	
Sat	18			1st Sh. I.	6 55	
		3 28	○ Full Moon	1st Tr. I.	7 24	11 5·9
				1st Sh. E.	9 9	
				1st Tr. E.	9 36	
Sun	19	21	Conjunction of Moon and Jupiter 6° 57′ S.	1st Oc. R.	6 47	11 1·9
Mon	20	18	Conjunction of Moon and Saturn 7° 41′ S.			
		9 46	Occultation of 104 Piscium (6½)			10 58·0
		10 43	Reappearance of ditto			
Tues	21			3rd Sh. I.	9 28	
		12 26	Occultation of B.A.C. 782 (6½)	3rd Tr. I.	11 21	
				3rd Sh. E.	12 7	10 54·1
		12 51	Reappearance of ditto	3rd Tr. E.	13 30	
				2nd Ec. D.	16 21 16	
Wed	22	9 35	Occultation of τ^1 Arietis (5)	1st Ec. D.	17 4 52	10 50·1
		10 33	Reappearance of ditto			
Thur	23			2nd Sh. I.	10 44	
				2nd Tr. I.	11 29	
				2nd Sh. E.	13 26	
				2nd Tr. E.	14 5	10 46·2
				1st Sh. E.	14 20	
				1st Tr. I.	14 42	
				1st Sh. E.	16 35	
				1st Tr. E.	16 54	
Fri	24			1st Ec. D.	11 33 41	10 42·3
				1st Oc. R.	14 5	
Sat	25	23 8	☾ Moon's Last Quarter			
		12 35	Occultation of 1 Geminorum (5)			
		13 34	Reappearance of ditto	1st Sh. I.	8 49	
		15 40	Occultation of 3 Geminorum (6)	2nd Oc. R.	8 52	10 38·3
				1st Tr. I.	9 8	
		17 3	Reappearance of ditto	1st Sh. E.	11 4	
		17 31	Occultation of 6 Geminorum (6)	1st Tr. E.	11 20	
		18 43	Reappearance of ditto			
Sun	26	11 5	Conjunction of *d* Geminorum (6)	1st Oc. R.	8 31	10 34·4
		11 33	Reappearance of ditto			
Mon	27	13 40	Near approach of *g* Geminorum (6)			10 30·4
Tues	28			3rd Sh. I.	13 30	
		6	Conjunction of Mars and Mercury 0° 6′ S.	3rd Tr. I.	14 37	10 26·5
				3rd Sh. E.	16 8	
				3rd Tr. E.	16 48	
Wed	29					10 22·5
Thur	30			2nd Sh. I.	13 22	
				2nd Tr. I.	13 45	
				2nd Sh. E.	16 4	10 18·6
				1st Sh. I.	16 14	
				2nd Tr. E.	16 21	
				1st Tr. I.	16 25	

THE PLANETS FOR SEPTEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	9 58 9	N. 13 55	5″·6	20 10·2
	9th	10 35 55	N. 8 46½	5″·0	23 36·4
	17th	11 44 13	N. 3 22	4″·8	23 59·7
	25th	12 35 12	S. 2 55	4″·8	0 16·4
Venus ...	1st	11 36 24	N. 3 59	10″·0	0 52·2
	9th	12 12 30	S. 0 5½	10″·2	0 56·7
	17th	12 48 33	S. 4 12	10″·4	1 1·2
	25th	13 24 57	S. 8 13½	10″·6	1 6·0
Jupiter ...	1st	1 10 56	N. 5 49½	44″·8	14 24·5
	9th	1 8 20	N. 5 32	45″·6	13 50·4
	17th	1 5 9	N. 5 11	46″·2	13 15·8
	25th	1 1 32	N. 4 48	46″·6	12 40·7
Saturn ...	1st	1 49 51	N. 8 26½	17″·4	15 3·3
	9th	1 48 33	N. 8 17	17″·6	14 30·5
	17th	1 46 55	N. 8 6½	17″·8	13 57·5
	25th	1 45 0	N. 7 54	17″·8	13 24·1
Neptune ...	13th	3 48 52	N. 14 21	...	15 15·0
	29th	2 47 45	N. 14 15	...	14 10·9

Mercury rises about an hour and a half before the sun on the 1st, the interval decreasing.

Venus is too near the sun to be well observed.

Jupiter rises about an hour after sunset at the beginning of the month, the interval decreasing.

Saturn rises about an hour and a half after sunset, on the 1st, the interval decreasing.

TO CORRESPONDENTS.

All communications should be addressed to the Editor.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

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The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 214. OCTOBER. 1880.

ASTRONOMY IN ITALY.

(Continued from page 217.)

On account of the absence from Rome of M. Respighi his reply to our inquiries respecting the actual state and operations of the observatory of the Capitol was too late to be used in the last number of the *Astronomical Register*. We therefore now add some further particulars from the letter he kindly sent us of 21st August. In 1876 the following gentlemen were appointed assistants; 1. Dr. Alfonso Di Legge, 2. Dr. Francesco Giacomelli, 3. Dr. Don Antonio Prosperi. The annual sum (only about £125) allowed for repairs and other expenses, leaves nothing for printing, so that the observations mostly appear in the transactions of the Royal Academy of the *Lincei*. The most important works of recent years are: Determination of the latitude of the observatory, Memoir in 1877; Catalogue of mean declination of 1463 stars, from 1st to 6th mag., between the parallels of 20° and 64° N. This is founded on 30,000 observations, from March, 1875, to April, 1877, and part was published in 1878, the whole is now in the press; Catalogue of 1000 stars, first to sixth mags., between the pole and 64° N. decl., and from 20° N. to the equator. These observations, begun Dec., 1878, already over 20,000, half being direct and half reflected, are nearly finished, and the reductions also in part completed. Observations of the horizontal diameter of the sun, regularly made from 1875 to the present time. Those for 1875, 76, and 77 are already published, and those for 1878 and 79 are in course of publication. These observations are intended to supply data for the determination of the question of the supposed variations of the sun's diameter, on which subject M. Respighi has in times past made and published researches. From October, 1869, the daily observation of the chromosphere of the sun, and protuberances all round, with drawings, has been carried on, and up to the present time there have been made 1960 complete profiles, not including those only partial, and special drawings of extraordinary phenomena. M. Respighi says, "Our observations now extend over a complete sun-spot period, and they prove plainly the correlation of the protuberances with the spots and with the faculæ, and the common dependence of all on one and the same cause, or on a periodical disturbance of the sun's surface." It should be remembered that systematic observations of the entire solar contour originated at this observatory. Seven memoirs have already appeared on these observations and the eighth is ready for printing. Other work of a special character has been done by the astronomers of the Capitol in connection with the geodesical commission for the measurement of the degrees of the central European meridian, and the difference of longitude between the observatory of the Capitcl and that of Brera in Milan, and observations are made of comets, planets, eclipses, occultations, stellar spectra, scintillations, &c. Meteorological observations are carried on, for which there is good apparatus, magnetic observations are made 12 times a day for the study of the possible correlation between the solar phenomena and terrestrial magnetism. M. Respighi has kindly sent us some of the publications of his observatory, which we may have another opportunity of noticing. The above extracts will give an idea of the activity and zeal which characterises this establishment and the valuable and important results for which we are indebted to it.

Naples.—The present observatory, which replaced that of the convent of San Gaudioso, where Casella (1798—1807) and Zuccari (1812—1817) observed, began to be constructed on the 4th November, 1812, by Murat, then king of Naples, on an elevation called *Capo di Monte.* After an interruption occasioned by political events, it was resumed by Ferdinand IV. in 1815, the plans being made by the celebrated Piazzi, and at the end of 1819 the whole was completed, and C. Brioschi commenced his labours as director. From the terraces of the observatory, situated on the summit of the hill of Miradois, a view is commanded of the sea, the city, and the ever-smoking Vesuvius. The building, of a rich and imposing style of architecture, is surrounded by a vast garden, which also encloses the residence of the astronomers. Brioschi had been chosen by Piazzi and Oriani from the young astronomers who had been students at Milan. Instruments had been obtained from Germany by the assistance and advice of Baron de Zach. They were a meridian circle, a meridian telescope and two repeating circles, all by Reichenbach; two equatorials, one by Reichenbach and Fraunhofer, 6·9-in. aperture and 9·9-ft. focal length. Two clocks by Arnold and Berthoud, a telescope by Amici, and other instruments. Brioschi published a volume of *Astronomical Commentaries* in 1824—26, and determined the position of his observatory; he was also preparing a catalogue of stars when he died 7th February, 1833. His successor was Capocci, one of his assistants, who executed the XVIIIth hour, R.A., of the Berlin charts, observed and calculated the orbits of various comets, and published some notes on Saturn's ring and the solar spots. After 1843 other occupations distracted Capocci from his observatory work. He died in 1864, and A. de Gasparis was called to the directorship, under whose energetic administration the observatory of Capo di Monte revived from its state of neglect. M. A. de Gasparis was born in 1819, and was induced by M. Capocci to study mathematics at the university of Naples. He entered the observatory in 1842 as assistant, and taking possession of the small equatorial of Fraunhofer, gave himself to the observation and search of new planets, of which he was the discoverer of nine. He also published a number of memoirs on the method of determining their orbits, and numerical tables to abridge the calculation. In a memoir in 1871 he describes a method of finding the true ellipticity of the orbit of a double star, which differs from that of Sir J. Herschel (1832). M. de Gasparis further promoted the science by gathering around him youthful astronomers, whom he instructed and animated by his example, and who in their turn became workers. A series of important labours was initiated at the observatory of Naples, which to this day continue to be published in the *Memoirs of the Royal Academy of Physical and Mathematical Sciences* of that city. The difference of longitude between Rome and Naples was determined in 1869, by M. E. Fergola at Naples, and the P. Secchi at Rome, by the observation of 108 stars in ten nights, and the exchange of 134 series of signals, and found to be 7m. 6·247s., with a probable error of ±0·027s. The unsymmetrical form of the repeating circles used by Brioschi, in spite of all his pains, left the error of flexion uncertain, and M. Fergola was obliged to employ the same instrument. In revising the latitude he therefore adopted the method of Talcott, which consists of the observation of the difference of zenith distance of pairs of stars which pass the meridian both north and south at nearly equal distances from the zenith. This method requires a precise knowledge of the declinations of the stars, but the flexion remains the same, and the difference of declination is micrometrically measured without reference to the graduation of the circle. The observations are 850 in number, in 52 pairs of stars, in two series. 1. From January to April, 1871, 2. From September to December of the same year. They gave for the latitude of the centre of the western dome of the observatory 40° 51′ 45″·41, less by 1″·2 than the old result of Brioschi. The longitude of Palermo was required by the engineers who were at work on the topographical chart of Sicily. It had been determined in 1838 by Antonio Nobile, by the simultaneous observation of some shooting stars. It was again determined by Nobile and Tacchini in 1869—70, and the difference of longitude between Naples and Palermo was found to be 3m. 35·823s., with a probable error of +0s.·013, being 0s.·5 less than by the observations of 1838. In 1875 M. Nobile published two memoirs on double stars. The first is the measurement of angles of position of various systems, previously measured by Struve or Dembowski. These were made with a telescope 4·3-in. aperture and 6·7-ft. focal length by Merz. The second describes a new method of finding the mutual distances of multiple stars, founded on a suggestion by De Gasparis in Nos. 1177 and 1201 of the *Astronomische Nachrichten.* It consists in giving the parallactic movement of the telescope a motion rather more slow or more rapid than the diurnal motion (the exact measurement of which is made by a contrivance of Nobile) so that the difference of the two components in R.A. is made apparent by their passage over the same wire, the difference of which passage can be increased at pleasure by regulating the clock movement. This method has given satisfactory results. M. de Gasparis has long desired to replace the old instruments of Reichenbach, by others more adapted to the requirements of the present day. When M. Rayet was in Naples, a fine meridian circle of Repsold, telescope 6·4-in. aperture and 6·6-ft. focal length, was placed in the eastern hall. Its circle, 3·3-ft. diameter is read by four microscopes, and it has collimators and meridian marks for measuring its horizontal flexure. The micrometer has movable wires which can be either made light in a dark or dark in an illumined field. Close by is an excellent clock by Grimaldi. M. Fergola intends to use this instrument for the observation of a catalogue of fundamental stars. The equatorials of Reichenbach and Merz should likewise be replaced by similar instruments of greater power. The telescope by

Fraunhofer, 6·9-in. aperture, already mentioned, has been mounted equatorially on the German plan, and is to be placed in a tower built on the north of the observatory.

In a letter of May 7th, 1880, M. de Gasparis informs us that the old meridian circle of Reichenbach has been altered and can now be inverted, and is adapted for the absolute determination of R.A. and Decl., so that the observatory has now two effective meridian circles. The dome for the 7-in. telescope and other apparatus, kindly presented by Mr. Bishop, has been completed. Two separate magnetic stations, with Gauss instruments, have been established. The meteorological station, apart from the observatory, has been furnished with new apparatus by the best makers. M. Nobile is continuing his researches on double stars. Professor Fergola has observed Mars with the Repsold circle, during its last opposition, for the solar parallax. The assistants, MM. Contarino and Angelitti, have made a commencement of the determination of the R.A. of a zone of stars of the same exactness as the most recent catalogues. Since 1878 various astronomical works have been published. Out of a number of memoirs kindly sent us by M. de Gasparis, are the following: "Development in series according to powers of the time of the three derivatives of the perturbing function"; "A similar development of the inverse function of the cube of the variable distance of two planets"; "On some derivatives, and a specimen of a calculation for planetary perturbations"; "On the variation of the elliptic elements in planetary orbits." The above published in 1879. "Development in series of the perturbing function according to powers of the time"; "On the variation of the radius vector of a disturbed planet during an infinitesimal time"; "On the variation of the differential of the square of the distance between two planets, occasioned by the perturbing influence of a third planet"; "On a relation of the distances in the problem of the three bodies"; "On the determination of the Right Ascensions of stars in zones," by Contarino and Angelitti. The above, published in 1880, are all by M. de Gasparis, except the last. Besides, he has published two auxiliary tables useful in the calculation of orbits. It was a happy incident that his meeting with Capocci in 1839, in the mountains of the Abruzzi, his native place, changed the destiny of De Gasparis (he was originally designed for the priesthood), and led to his connection with the observatory of Capo di Monte, where he has rendered such valuable service to astronomy, both observational and theoretic. The front portico of the edifice bears on its entablature the inscription: FERDINANDUS I., ASTRONOMIÆ INCREMENTO—ANNO MDCCCXIX, and assuredly those who, like the present director and his assistants, have so ably and zealously laboured for the advancement of the science, have no reason to be ashamed of it.

PALERMO.—The Sicilians boast of having been the first among the peoples of Italy who cultivated astronomy. Among the names cited in support of this are F. Maurolico, (1494—1575), professor of mathematics at Messina; B. Hodierna (1597—1660), who published the first tables of Jupiter's satellites; and Leonard Ximenes (1717—1786), founder of the observatory at the college at San Giovannino in Florence, which subsequently belonged to the Scuole Pie. But the honour of founding the first observatory in Sicily, under a sky which in Europe has no rival for transparency, was reserved for Piazzi, born July 16th, 1746, at Ponte in the Valtelina (Lombardy). He was of a feeble constitution, and being early destined for an ecclesiastic, entered in 1765 the monastic order of the Théatins. He studied at Turin under Beccaria, and at Rome under the R. R. P. P. Jacquier and Le Sueur, editors of the well-known edition of the Principia. He became professor of philosophy in several places in succession, but too free expression on certain theological points made him many enemies. At last, whilst professor of theology at San Andrea at Rome, he was, in 1780, by the recommendation of the R. P. Jacquier, transferred to the chair of the higher mathematics at the academy of Palermo. The establishment of a chair of astronomy, to which the viceroy of Sicily, Prince Caramanico, nominated him, required an observatory, and in 1787 he visited the chief observatories of Europe, familiarised himself with practical astronomy, and ordered the necessary instruments. He spent eighteen months at Paris with Lalande, taking part in all his observations, after which he went to Greenwich, where he observed with Maskelyne the solar eclipse of 1788, on which he presented a memoir to the Royal Society, his first astronomical production. In August, 1789, the vertical circle for the Palermo observatory was completed by Ramsden, and the old tower of the royal palace was by the interest of Prince Caramanico (himself an astronomical amateur of some note) fixed upon as the site of the observatory, and apartments in the palace were assigned to the director. The tower of San Ninfa, of Arab construction, is the highest in the city, and commands an almost unbroken horizon. Its walls are 17-ft. thick at the base, and six feet at the summit. The circle stands on a pillar which rests on the walls, in a dome supported by marble colums. It is double, and five feet in diameter, and can be read by two microscopes to single seconds. The telescope is 3-in. aperture, and 5-ft. focal length. There was also a transit instrument, an excellent clock by Mudge, and other telescopes, clocks, and accessory instruments. Star catalogues of precision were then much wanted, and were undertaken at the same time by Baron de Zach, Cagnoli, and Piazzi. Whilst engaged in this work, on 1st January, 1801, Piazzi discovered Ceres.

> Piazzi, per cui del secolo
> La prima ora novella
> Mirò nell' etra accendersi
> Non pria veduta stella.* (*Guadagnoli.*)

On the 24th, writing to Oriani the particulars, he said that the absence of any nebulosity, and its very slow and uniform motion, made him think it might be something better than a comet, but he would not venture to make this conjecture public. Very shortly, however, it was recognised as a planet, and its orbit was calculated by Oriani, Burckhardt, and Gauss. This discovery brought Piazzi a pension from the king, in whose honour the little planet was at first called Ceres Ferdinandea. The money intended for a medal to commemorate it was devoted, at the astronomer's request, to the purchase of a small equatorial by Troughton, which was employed for the observation of comets. Piazzi traced a meridian line in the cathedral of Palermo. Aided by M. N. Carioti, and later by N. Cacciatore, his star observations and reductions were finished in 1802, and the following year appeared the magnificent volume in folio of his first catalogue of 6748 stars. For this he obtained, in 1803, the Lalande medal. It was soon found desirable, however, to re observe the fundamental stars of Maskelyne, which was done by N. Cacciatore, whilst Piazzi observed the sun, and in 1806 a new catalogue of 120 stars, and another of 100 fundamental stars were published in the sixth volume of the Royal Observatory of Palermo. This preliminary work was followed by the revised edition of the great catalogue in 1814, and the fruit of 22 years of continuous labours again obtained for its author the great astronomical prize of the Institute of France. The determination of the solstices, and of the obliquity of the ecliptic, &c., resulted also from Piazzi's observations, and formed the subjects of various memoirs. His health began to fail with advancing age; notwithstanding, he was charged in 1817 with directing the construction of the observatory of Capo di Monte. The brief periods of his residence in Sicily after that date were occupied with the publication of his lessons on astronomy, and researches on the proper motion and the parallax of stars. In these labours he was overtaken by death on July 22nd, 1826.*

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* Piazzi, owing to whom, since the first hour of the new century, I gaze upon a star unseen before, kindled in the sky.

After 1817 the direction of the observatory of Palermo was entrusted to M. N. Cacciatore, the most deserving and the most laborious of the assistants of Piazzi. Originally intended for the priesthood, he went to Palermo in 1799 to study Greek, and soon became one of the most active pupils of Piazzi, in whose labours he took part after 1800. When he became head of the observatory in 1817 he undertook a determination of the refraction proper to that climate, and continued the observations of his predecessor on the equinoxes and the planets in opposition. In the revolution of 1820 the palace and the observatory were attacked by the mob, papers were scattered and burned, and the astronomer was only at the peril of his life enabled to save the instruments and observatory from utter destruction. This catastrophe, which Piazzi describes in a letter to Oriani, hindered the prosecution of work till 1822. Many of the dispersed papers, however, were recovered, and N. Cacciatore published, in 1826, observations on the oppositions of planets, of the sun, the proper motions of 173 stars, and of the comets of 1821-22-25 (Encke's), and the great comet of 1828. He then resumed the observations of the stars of Piazzi's catalogue, and traced the differing values of the obliquity of the ecliptic deduced from the winter and summer solstices to the action of temperature on the instruments. He arrived at this conclusion after placing thermometers on various parts of Ramsden's circle. After carrying on these researches till 1837, M. Cacciatore was disabled from work by a serious illness, and he died 28th Jan., 1841. His son, Gaetano Cacciatore, who had been his assistant for some years, succeeded him in the year following. The long illness of his father, and the indifference of the government of the Two Sicilies to astronomical researches, which left the establishments at Naples and Palermo without needful means, suspended almost all the operations of the observatory. When the revolution of 1848 took place, G. Cacciatore, who as deputy of the parliament had taken part in the expulsion of the Bourbons, was obliged on their return to quit Palermo, leaving the observatory under the care of M. D. Ragona, who was then second assistant. Appointed director about the end of 1850, M. Ragona obtained permission from the king Ferdinand to finish his astronomical training in the observatories of Germany, and was also furnished with the necessary credit for the restoration of the observatory. He left Palermo in the beginning of 1851, and stayed successively in the observatories of Northern Italy, at Vienna with Littrow, and finally at Berlin with Encke, who had gathered around him quite a constellation of pupils. By the advice of Encke M. Ragona ordered a meridian circle of Pistor and Martins, and an equatorial of Merz, and returned in 1854 to Palermo, whilst the instruments were being constructed, and gave himself to the observation of some southern stars. The meridian circle arrived in 1857; it has an objective of 5·1-in. aperture, and 6½-ft. focal length. The two circles are 3-ft. in diameter, graduated on silver, and read by four microscopes. With this instrument the latitude of the observatory is 38° 6′ 42″·67, less by 1″.33 than by Piazzi's determination. M. Ragona also continued with this circle the observation of southern stars between —25° and —45° decl. These results are published in three volumes in the *Giornale Astronomico e Meteorologico del R. Observatorio di Palermo.* Palermo, 1855-60.

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* Those who have used the late Admiral Smyth's delightful "Celestial Cycle" know how much use has been made in that work of Piazzi's observations. The Admiral was a personal friend of Piazzi, and has given a sketch of his life in the first volume of the Cycle.

The revolution of 1860 brought back M. G. Cacciatore to the observatory. His first care was the placing in position of the great equatorial which had arrived some years previous. It is one of the *chefs d'œuvre* of Merz, mounted on the German plan. The objective is 9·4-in. aperture, and 14·5-ft. focal length. The tube is of wood, of a form slightly conical, and to guard against flexure is furnished with two lateral rods carrying movable counterpoises. The installation of the equatorial was effected in the beginning of 1865, and it has since been daily employed in solar observations. M. Cacciatore also originated the *Bollettino Meteorologico del R. Observatorio di Palermo*, which, together with meteorology, embraced up to 1872 a large number of astronomical memoirs. In September, 1863, M. Tacchini arrived in order to replace M. Ragona, who is now at the observatory of Modena.

M. Tacchini, born at Modena in 1838, presided over the observatory there since Sept., 1859. He lost no time in applying the equatorial of Merz to the investigation of the phenomena of the sun. For the solar eclipse of Dec. 22nd, 1870, a direct vision spectroscope with five prisms, by Taubner, of Leipsic, was procured, but it arrived too late for the eclipse. The great advantage of the association of observers in different places occurred almost simultaneously to P. Secchi and M. Tacchini, and led to the formation of the Society of Italian Spectroscopists. The association comprises the observatories of Palermo, of the Collegio Romano, of the Capitol, of Padua, and of Florence. It was organised at Rome at a conference in Oct., 1871; and soon after the Italian government accorded the funds necessary for the annual publication of the *Memorie della Società degli Spettroscopisti Italiani.* Observations made after uniform methods and with similar instruments have since then been uninterruptedly prosecuted, and have led to results of importance. Discoveries and discussions on solar physics have appeared in the five volumes published under the care of M. Tacchini and the late P. Secchi. M. Tacchini went to India in 1874 to observe spectroscopically the transit of Venus, and published an account of it at Palermo in 1875. Since his transfer to the Collegio Romano, as already noticed, the directorship of the Palermo Observatory has devolved on Prof. Cacciatore, who informs us in a letter (July 12th) that the principal work carried on of late years is solar spectroscopy. He says—"A splendid equatorial by Merz, with the fine sky of Palermo, has attached some importance to these observations, which have been regularly published. Fundamental astronomy has not been neglected, although for many years the want of assistants has restricted it to very narrow limits. Meteorology is much cultivated; and M. Cacciatore has removed the instruments to a better site, about half-a-mile outside of the city, where the observations have been made from January, 1880. He is rejoiced to be able to state that the government has sanctioned a new arrangement of the observatory work, which is to be divided into three sections. 1.—Fundamental astronomy. 2.—Physical astronomy. 3.—Meteorology and magnetism. The *personnel* is to consist of the director, an astronomer for fundamental astronomy, an astronomer for physical astronomy, an assistant for meteorology and magnetism, two computing assistants, a keeper, and two attendants. Prof. Cacciatore hopes that with the new year the observatory may commence its routine of labours in these various branches, and be enabled to publish the results annually. This is welcome intelligence; and we trust the promising programme will soon become a reality.

We have endeavoured to give as full an account of astronomy in Italy, in past and present times, as our limits allowed; and as we have been indebted for the greatest part to the work named at the head of this article, in tendering our thanks to its author for his interesting history, we can but commend its perusal to those who desire fuller information. Amply sufficient, however, appears in this *précis* to show that Italy, to which all Europe and the world are under so great obligations in every department of literature, has also produced many men of high distinction in astronomical and mathematical science, and in the present generation possesses some who are worthily maintaining the honourable traditions of the past. Considering the extraordinary difficulties which official astronomers have not unseldom had to encounter in that country, it will be felt that great credit is due to them for the much which, notwithstanding, they have effected. We wish we could speak of private astronomical establishments, such as, especially of late years, have been multiplied in other countries; but we are not aware of the existence of any except that of the Baron Dembowski, of double-star celebrity, at Gallarate, near Milan, who during the 28 years in which he has carried on his micrometrical measurements used at first a telescope of 5-in. aperture, and since 1864 one of 7-in. Meteorology, however, is much cultivated. It seems to be the chief, if not the exclusive, work of M. Ragona, at Modena; of Pigorini, at Parma; of P. M. Garibaldi, at Genoa; of A. Palagi, at Bologna; of the observers at the monastery of Monte Cavo (the highest mountain of ancient Latium, where P. Secchi established two observatories, one at Grotta Ferrata, 1083 feet above the sea, the other at the top at an altitude of 3127 feet. Shooting stars are also observed here); of Meneguzzi, at the observatory of the Seminary at Venice, where a volume of observations is published every year; and in particular of P. Denza, at the monastery of the Barnabites at Moncalieri, which is the central observatory of meteorology of all North Italy and the Alps. There is also the observatory of M. Palmieri at Naples, for meteorology, and the phenomena of Mount Vesuvius. Besides the above-mentioned there is a large number of meteorological stations in Italy in correspondence with Moncalieri.

We sincerely trust that what M. Rayet says about the government and the learned men of an united Italy vying with each other to restore its due rank to the land of Galileo may not be a little too *couleur de rose*. Regarding its men of science we entertain no misgivings. But the government, whether rightly or wrongly this is not the place to inquire, deems the maintaining of powerful armaments to be a necessity; and the consequent expense very likely prevents its using as much liberality towards its astronomical establishments as it would desire. Heavy taxation must also have a depressing effect on the exertions of private individuals. In this country, where, though sometimes neither sun nor stars in many days appear, star-gazers abound, what would we often give for

> —the azure gloom
> Of an Italian night; where the deep skies assume
> Hues which have words, and speak to us of heaven.*

Yet how comparatively few telescopes are directed to those inviting skies! Where we are writing these lines there is hanging before us a fine photograph of the Arcetri Observatory, taken from the terrace of Torre del Gallo, linked with the memory of "the starry Galileo, with his woes;" and dimly discernible on the left is a villa associated to ourselves with many memories of early days. The observatory existed not then, and it has since lent an additional feature of interest to the beauty of the scene viewed from that terrace, which, perhaps, has few equals in attractiveness anywhere. A Latin inscription on a house near this tower commemorates it as the residence of Galileo from Nov. 1st, 1630 till Jan. 8th, 1642. He is called CŒLI MAXIMUS SPECTATOR, ET NATURALIS PHILOSOPHIÆ RESTITUTOR, SEU POTIUS PARENS. In this house he died on the last-mentioned date; and the traveller is admonished to revere the sacred genius of the place.

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* Childe Harold.

We have thought it superfluous to refer to the well-known discoveries and inventions of the illustrious "Tuscan Artist." But if his native land is to regain in science a rank worthy of his name, the spot of all others from Turin to Palermo associated with it must be freed from the reproach which still rests there. From that terrace of the Torre del Gallo the great astronomer viewed the heavens with his famed "optic glass"; but the *genius loci* mourns the tower's neglected state, and every visitor echoes the lament.* And close by, where a fine telescope—compared with which the best of Galileo's making would now be a scientific toy—made by another of Italy's gifted sons, is worked by an able astronomer, it is under the disadvantages already alluded to. In his address of Sept. 26th, 1869,† at the inauguration of the new provisional observatory, the lamented Donati expressed the hope that the new Florentine Observatory might contribute effectively to the progress of the science, so that

> "Vengano i veri frutti dope 'l fiore."

Of *flower*, indeed, there has been no want; and if the prospect of fruit in proportion has not yet been realised, who is to be blamed? Without going into this question, but gladly recognising what has been so far accomplished, and willingly crediting the government of Italy with the desire of effacing this blot, we trust it will soon find itself able to give effect to it, and that the observatory of Florence, as well as those of Milan, Naples, and Palermo, may in no respect remain behind the requirements of the age, and short of the aspirations of lovers of astronomy, not only in Italy, but all other countries.

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* See "A Visit to the Torre del Gallo," *Astronomical Register*, vol. xi., p. 104.

† *Astronomical Register*, vol. vii, p. 215.

JUPITER.

By Prof. G. W. Hough, of Chicago.

The great red spot on the southern hemisphere of the planet does not appear to have changed in size, shape, or position since the past opposition. It ought, therefore, to afford data for determining with great precision the rotation period of Jupiter.

In order, however, that observations shall have any scientific value, a method of observing should be adopted which is able to afford reliable results. A mere estimation when the spot is central on the disc will hardly meet the demands of modern science. To ascertain the position of any object on the disc of a planet, which shall be comparatively free from general bias, it should be submitted to some sort of measurement with the micrometer or other appliance. In the case of the spot in question, in order to properly reduce measures of its distance from the central meridian of the planet, it is necessary to know with some degree of precision the latitude and magnitude of the object. The length, as every observer knows, varies from zero when coming on the disc, to its maximum value when on the central meridian, the *apparent* centre, therefore, never coincides with the true centre, except in the *latter* position.

For ascertaining the time of central passage, micrometer measures of the apparent centre of the spot are to be preferred above all others. The measures should be made in pairs of, say, three bisections each, as follows:

Centre of spot and ♃'s preceding limb.
Centre of spot and ♃'s following limb.

One-half the difference will be the distance of the *apparent* centre of the spot from Jupiter's central meridian at the mean of the times.

The position of the planet's equator is given in Marth's ephemeris—if this is not at hand the north edge of the equatorial belt may be used for setting the micrometer.

Four sets of measures, which will require not more than one half-hour's observing, will furnish the time of central passage, with a probable error of less than 0·5 minute.

The measures may be made when the spot is in any part of the disc, but in order to reduce the error incident to an erroneous value of the planet's diameter, and length of the object, it is advisable to make the observations near the central meridian. When the measures are referred to only one limb of the planet, irradiation may affect the result to the amount of three or four minutes of rotation time.

A second method is to use a chronograph recording the transits of the two limbs of the planet, and the preceding and following ends of the spot. A set of ten transits over one wire, made in this way, is equal in point of accuracy to one set of micrometer measures.

In case the observer has no chronograph driving-clock for his equatorial, the following method may perhaps be found useful. Place two micrometer wires 10 or 15 seconds of time apart, and at right angles to Jupiter's equator, then observe the transits of the limbs and the centre of the spot on alternate wires by "eye and ear." Although this method is not susceptible of very accurate results in observations of this kind, yet it may be found of value in the absence of a better apparatus.

The direct estimation of the time of central passage, either with the aid of micrometer wires or without, is not reliable, as may be ascertained by consulting Marth's discussion of last year's observations. The reason is obvious. A single second of arc, corresponding to 5·5 minutes rotation time, is a quantity difficult to estimate on the disc of Jupiter, even with a first-class telescope and high magnifying powers.

The following constants may be found useful in reducing observations. They are referred to the mean distance of the planet from the earth.

Distance of centre of spot from Jupiter's equator, south	6″·94
Length of chord seen on centre of disc	12″·24
Greatest breadth of spot	3·56
Time in crossing Jupiter's central meridian	1h. 06·2m.
Time from central passage when wholly on disc	1h. 55·8m.
Apparent length when wholly on disc	4″·18

These values are the results of observations made on eight nights during the opposition of 1879. Measures on three nights, during June and July of the present year, give the following: Distance from Jupiter's equator, south, 6″·91; Length, 11″·60; Breadth, 3·″58. The observations so far indicate that there has been no change in the latitude, length, or breadth of the spot since September of last year, neither has there been any progressive or other change in longitude during the same period.

The following ephemeris has been computed for the meridian of Greenwich, and gives the Greenwich mean time when the centre of the spot is on the apparent meridian, at right angles to and bisecting the equatorial diameter of the planet.

The adopted time of sidereal rotation is 9h. 55m. 34·5s. Corrections have been applied for aberration, equinox, annual parallax, and defective illumination. Observations of the central passage of the spot may, therefore, be directly compared with the ephemeris.

1880. Date.	Greenwich M.T.		Date.	Greenwich M.T.	
	h.	m.		h.	m.
July 16	16	02·2	Sept. 2	15	24·2
19	13	31·2	3	11	15·2
21	15	09·0	5	12	52·7
24	12	38·2	7	14	30·3
26	14	15·9	8	10	21·3
28	15	53·6	9	16	07·6
29	11	44·6	10	11	58·6
31	13	22·4	12	13	36·0
Aug. 2	15	00·3	13	9	26·5
5	12	29·1	14	15	13·3
7	14	06·7	15	11	04·2
9	15	44·4	17	12	41·7
10	11	35·4	19	14	19·2
12	13	13·2	20	10	10·1
14	14	50·8	21	15	56·6
15	10	41·8	22	11	47·5
17	12	19·4	24	13	24·9
19	13	57·1	25	9	15·8
21	15	34·7	26	15	02·5
22	11	25·5	27	10	53·2
24	13	03·1	29	12	30·7
26	14	40·8	Oct. 1	14	08·1
29	12	09·5			
31	13	46·9			

THE ROCK-THERMOMETERS OF THE CALTON HILL, EDINBURGH.

By Piazzi Smyth, Astronomer-Royal for Scotland.

Forty-four years ago a remarkable set of thermometers was established for observation in the solid porphyritic rock of the Calton Hill. No such thermometers had ever been seen in this country before, nor have many like them appeared since; for the largest was no less than 26 feet in length from the centre of the bulb to the beginning of the scale, and that a scale with degrees two inches long, and reading off with facility to hundredths of a degree Fahrenheit. There were other thermometers in the series of 12, 6, 3, and 0·3 feet in similar intermediate tube-length, besides smaller ones for air temperature; the bulbs of all the former being sunk and enclosed in the earth at depths in feet corresing to the lengths above named, in a vertical bore-hole worked down into the hard rock, and then tamped with sand after the insertion of the thermometers; while an appropriate little wooden house was erected over the mouth of the hole, and a rigorous system of observation and computation of the observations for various minute corrections of high science was commenced.

Surely that must have been the age of thermometers, or some most remarkable men must have lived and worked then connected with thermometrical science carried to an ultra degree of refinement may be the remark of many a reader! and a very true remark too; for was it not the age of Arago in France, Quetelet in Brussels, James David Forbes in Edinburgh? Yes, indeed; and it was also, perhaps, the very culmination of the thermometrics of the schools in their application to physics of chamber science in all its most intensely terrestrial applications.

Professor J. D. Forbes, so well known and highly appreciated in that day for his accuracy in dealing with the minutiæ, even microscopic, of all heat observations, was the chief agent in establishing these best and grandest thermometers in the world, within the little territory of the Royal Observatory, Edinburgh, then under the rigid directorship of Thomas Henderson: and when the latter eminent man and his first assistant, Mr. Alexander Wallace, had carried on the observations for seven full years, the former professor took up the task of their computation first; and then the application of the reduced results to the particular physical laws which were of chief interest amongst learned men at that time,—namely, the time rate at which the wave of each year's summer heat travelled downwards into the ground, and the amount of absorption it experienced in so doing.

Great was the praise bestowed by his contemporaries on Professor Forbes' numerical values for these effects thus obtained. And he deserved it well; for had he not transcended in accuracy anything and everything accomplished in that line previously in any country? But, says a severe observer with the telescope, that line, namely, the statics, or even dynamics, of chamber natural philosophy temperature, is not practical astronomy! Why, therefore, should the readers of *The Astronomical Register* be troubled about it?

No reason at all, had the matter ended there, but it did not; for the reader's humble servant having then entered on the scene decided, notwithstanding that the ends originally proposed had been obtained,—decided, I say, to keep up the whole system of observation and careful instrumental corrections of those thermometers. They were all kept accordingly without a break, and soon after came the reward; namely, that thereby a puzzled astronomer was enabled to prove that a fluctuation in angular position, which the transit instrument in the Edinburgh Observatory had long been liable to, was not produced, as some local authorities had given out, by movements of the whole rocky Calton Hill, twisting and writhing in the sunshine; but chiefly by expansions and contractions in the piers and metal bearings of the instrument itself, in its own chamber.

"But in what is that better, more astronomical, or more fit to lay before students of *The Astronomical Register*, than Professor Forbes' earlier results? for these transit movements were also chamber affairs only, capable of entire elimination by employing proper metal, and stone work in mounting the instrument; and leading to no demonstration of new laws in Newtonian astronomy; no discovery of hitherto unknown existences among the heavenly bodies!"

True, most true! but then the course of observation with these most deeply searching thermometers did not end even there. Year after year continued to add its quota to the mass of records already accumulated, and then Sir William Thomson, of Glasgow University, began to look into the series with his ultra-high mathematical analysis. Soon, too, was he able to draw forth and publish some cosmical results of very transcendent order; such as, first, the amount of the earth's central heat, which is still every year escaping through the crust into space; and then an approximation to the age of that heat, or the number of millions of years since the whole earth was superficially, as well as internally, of at least a red heat, and an impossible *nidus* therefore for any form of either animal or vegetable life.

"Geological, not astronomical! All of the earth, earthy!" still objects many a straight-laced, practical astronomer. But yet we pray you have patience for a while, for the system of observing and recording those long and deeply rock-embedded thermometers was still kept up; and by the time that thirty years of records had been accumulated some strange variations, unlike anything that pure and abstract mathematical theory had devised, and rather pointing to a heavenly orb for possible explanation, began to manifest themselves. Yet what could they be?

About that absolute date the reading public had began to be agitated in rather an absurd way touching the length of time that the sun's light and heat were going to last. Because, simply, Sir William Thomson had introduced into his calculations what is practically little more than an abstract principle, so almost infinitely long and slow are the steps, namely. a confession of the necessity, to prevent an accusation of perpetual motion, that the sun must once have had a beginning, and would equally necessarily some day have an end,—though, as a good natural philosopher, he did not mean to assert that we were within thousands of millions of millions of years of either the one or the other,—therefore the writers of slashing articles in those self-sufficient literary magazines which swarm so in and about London in these latter days of the nineteenth century,—these gentlemen of the pen, overlooking the amount of time clearly insisted on, spread broadcast in polite society the most ridiculous stories that a professor in Glasgow was teaching the frightful dogma, of how we might awake any morning and find the sun dark, cold, and dead.

"Oh! and what are those changes which you have already acknowledged you find in the long series of the Edinburgh rock-thermometer observations? Is not the sun's heat, according to them also, dying out; and is not the sun day by day hastening to its end?" were the agonizing appeals put to me by well-to-do papas, mammas, and young ladies, who all of them were accustomed to read diligently their monthly quotas of anonymous metropolitan literature, with tales, and novelets, and little bits of sarcastic science-reviews cut and trimmed ready for their taking in.

Well! variations of the thermometrical readings there were from year to year; and they were not caused by escapes or passages of terrestrial heat, for the deeper *followed* the shallower thermometers in their dates of indication of such heat waves. The thermometers had, in fact, been warmed up by something outside the earth, and what is there outside our globe to warm it up appreciably, and effectively, through all human history except the sun. And you say, I was categorically examined in 1867, that that warming influence of the sun on the earth has been steadily going down for several years! What more do you require in order to know that the dear and once mighty sun is dying before our very eyes?

But the year 1868 brought up the solar influences to a surpassing height once again, so that the failing witnessed for a few years previous was no part of a general and uniform decadence. In fact, by examining the whole series carefully it was soon shown, that the chief facts of the series alluded to formed no perceptible part either of our grand sun's general growth, or general decay, whatever they might be, through more or less millions of millions of years, but denoted merely the rise and fall of a most petty, though undoubted, undulation of solar temperature running through all its course in the short period of eleven years, more or less, and leaving the sun at the end of one, ready as a giant to run through the course of a second, a third, and almost as many more such cycles as you please. One such maximum as we have said occurred in 1868, a previous one in 1857, one before that in 1846, another earlier still (as gathered from air observations only) 1834, and another still more remarkable one in 1826.

On projecting all the observations according to their heights and the dates (after carefully eliminating the ordinary seasonal effects of what may be called the geometrical, or mechanical, summer and winter), the epochs of those super-annual heat-waves came out most conspicuously, forming *quasi* mountains of temperature, heaped on the earth from without; and as if to make them even more conspicuous, to prevent the most careless eye passing them by, or the most prejudiced refusing to note them, the coldest of all the intervening years was immediately on either side of these exceptional heat-waves. Sometimes the preceding, sometimes the following, cold period was the severer of the two, but there was always an effort, as it were, of nature to have two: and strangely close both of them in point of time, say within a year and a half or a little more, of the central occurrence of the heat-wave.

Did the sun's own direct and radiant heat fall (or fail) at the first cold period, rise again with the heat-wave, and fall again at the second cold period? I am not sure that it did, although the temperatures indicated by the rock thermometers followed that law precisely. But then earthly surface temperature is a very compound phenomenon. Such temperature, in the main, may be, as indeed it is and must be, ultimately referable to the sun; and the sun's own mode of acting may be nearly constant, but the earth's mode of receiving is most various. If, for instance, one of these eleven-year heat-waves falls due at the midsummer of any one country, and adds to the ordinary warmth of its annual seasonal occurrence, everyone there perceives and is impressed by the sultry heat of that year; but if it occurs, and there is no reason why it should not, at that land's midwinter, the remark is only, what a mild and proper season, how pleasant are the westerly breezes, and even the east have little of their usual sting in them.

Again, when the heat-wave bursts forth in, and at, the sun, some small part of it reaches the earth almost immediately as radiant heat; accompanied, too, as now proved again and again, by magnetic and auroral influences, as instant almost as light; but by far the greater part of the wave of heat is absorbed into the upper regions of the earth's atmosphere, becomes changed there into culinary heat with power of conduction and convection, but none of radiation; wherefore then, it simply joins the winds in their circuits, as they whirl about continually round and round the earth, spirally in the conjoined path of axial rotation and equatorial *versus* polar temperature; and though all the practical effect ultimately reaches the surface of the earth, and every part thereof profits thereby, yet some regions receive it earlier than others, and some in a very different manner from the others.

Thus, if the hot wave occurs in the December portion of the year, the radiant effect is felt almost exclusively in the southern hemisphere; and as that consists chiefly of ocean-surface, then extra evaporation and charging of the atmosphere with moisture, in various forms, is the first result. Next, that extra moisture being, in time, by the trade and anti-trade winds carried round to the northern hemisphere, produces there a cloudy summer; and a cloudy is a dull, if not also cold summer, by the most simple of all reasons, the exclusion of the direct rays of a warming sun.

But if the solar heat wave breaks out when the rays are most direct upon solid continents of dry land, the atmosphere is dried; its capacity for absorbing moisture and making clouds disappear is increased, and a sunshiny, hot summer takes place; though, if the clear skies and paucity of clouds last on to mid-winter, these continents will then lose far more temperature by cold radiation through the long nights, than gain by hot radiation through the short days. And once again, whereas the southern hemisphere is, by its superabundance of ocean, the chief evaporator for producing rains for the northern hemisphere, the South Pacific, the greatest of all the southern oceans, and the American continent, the first condenser which that moisture meets in its curvilinear path from S.W. and N.E., America feels the effects of a rise in the sun's radiant heat when he was beaming over the far-off wastes of the southern hemisphere, long before Europe does, whence, as everyone has been recently remarking with every telegram of the *New York Herald*, "America sends us our weather;" and, as the manufacturing and agricultural classes have also had good occasion to know of late, the general improvement of the weather after many bad seasons, together with the growth of the crops, the state of trade, the prosperity of the people began in America many months before it was perceivable in Great Britain.

But we are running on too fast; let us return, if you please, to the year 1868, and the hot wave therein, *i.e.*, hot in Scotland; and hot there chiefly because the summer was cloudless, the rain deficient, the sunshine constant, and therefore the grass burnt up, the springs dry, and the cattle on a thousand hills dying for very thirst. But all these latter features were more especially a consequence of the extra large wave of solar heat being felt there in the mid-summer season. Now, the existence of that wave, or extravasation of and from the sun, we have only as yet concluded from its believed indirect effects on earthly rock-temperature; and, of course, it is most desirable that direct observations should be appealed to on the sun itself. But what sort of observations shall they be? The meteorological observations of the various nations, so abundant in the present age, are full of notations and columns of numbers, which some persons appear to imagine are exactly what we are calling for; seeing that they are the readings of thermometers exposed freely in the sun, some of them with plain bulbs, some blackened, some enclosed in vacuous glass globes, and one or two extra zealous savants refer to Sir John Herschel's actinometers; while others are beginning to invent methods of making the sun produce day by day a charred or burnt-out passage in wood, showing how strongly, and when, he shone.

Some of these methods may be useful enough for indicating local terrestrial climate, but are absolutely valueless for determining the radiant heat of the sun itself in its own home, because an unknown amount of the earth's atmospheric absorption as well as atmospheric obstruction and atmospheric removal intervene within a very few miles above the observers' heads.

What, then, can we have recourse to, safe from terrestrial perversions? Optical observations of the sun's surface is the only means, if there are definite phenomena there to be observed; and if these phenomena do in size or shape indicate the activity of the sun's radiant powers. Now, all the world had long been acquainted with black spots which appear from time to time on the sun's surface, and know well that they are absolute, and telescopically tangible, things; they show something or other going on at the sun, and nothing in the earth's atmosphere can simulate them, or alter their places, sizes or shapes, so long as they are seen at all. But for centuries, if they were not looked on as accidental merely, no reasonable connection with changes in the force of the sun's radiation had been made out, and they were neglected. The moment, however, that M. Schwabe in Germany announced that they had a nearly regular period of eleven years, and M. Lamont at Munich, Wolf at Zurich, Sabine in London, and all the principal physical observers of every country further discovered that the magnetism of the earth, as a whole, was variable in the same period of years, and rose and fell with the average number of sun spots; and when the British Association observers, and more especially Prof. Balfour Stewart, now of

Owen's College, Manchester, traced particular magnetic storms on the earth to particular spots simultaneously appearing on the sun; when auroras, with their otherwise acknowledged power of disturbing the rainfall of the earth, were likewise in their frequency found to follow the law and the times of the sun-spots —then every thinking man perceived in them precisely the solar indicator which had so long been required.

An indicator only. Let us be quite within bounds on that point, for the sun-spots have no power in and by themselves to affect the earth's climate to any sensible degree. But they show that something else far more powerful is at work. Just as the live cinders falling through the fire-bars of a steam-engine's furnace cannot of themselves raise the steam, burst the boiler, or keep the engine performing heavy work; but the degree of rapidity in sequence with which such glowing embers may fall, will undoubtedly indicate something of the condition of the really effective coal fires above. When the fires, for instance, are banked down, and the steam-engine is still, few and rare will be the embers that find the way to fall through; but when one steamboat perchance begins to race with another, especially on American waters, and the stokers begin to stoke like mad and burn up every bit of fuel, and the bulk-heads of the cabins as well, then the live embers fall through fast enough; and if they are the only feature which circumstances allow us to view, some very sage conclusions may be arrived at, by those who observe them according to fixed principles of mensurational science.

Now, when it subsequently fell to my lot to discover the temperature cycles of the Edinburgh rock thermometers, and to prove that they were of the nature of, or born by, solar influences, I could not but be greatly moved on finding further that the whole periods of their cycling changes were almost exactly the same as that of the visible sun-spots. When *they* had had a cycle rather extra long, so had the thermometers; and if the sun-spots did not show anything analagous to the cold waves of the thermometers close on either side of the hot wave, that was because the cold waves were due to the earth's atmospheric influences, which do affect the thermometers, but cannot touch the sun-spots. The heat wave was, therefore, and had, indeed, all along been considered the chief result, and the most solar, too, of all the thermometrical demonstrations; and if it did not coincide in date with either the maximum, or the minimum, of sun-spots, it did what was more important; for it took place just such a length of time after the minimum, as, not the number of old spots accumulated, but the rapidity of formation of new spots from day to day, showed that the solar forces were then most active, and the sun's youth at that moment being renewed like the eagle's, to run a new career.

If, then, I was so confident in having obtained at last, not a theory of small changes from day to day, but a practical rule for the chief and leading phenomena of the weather of this country, extending, in an average manner, over many years, what use did I make of it for the good of the community at large?

Why this! In the thirteenth volume of the "Edinburgh Astronomical Observations" I published very conspicuously, in the early part of 1872, that the next very warm season would not take place before 1880, and that it would be immediately preceded by an extra cold season in the winter of 1878-9.

Who does not know now how well both of those long-shot, and apparently almost mutually contradictory, forecasts turned out. For when the pre-announced cold season did come on, and was so very cold and wet as to throw back agriculture with a loss, according to that potent genius, Mr. Mechi, of 150 millions of money in Great Britain alone, every one suffered more or less with the farmers. Everyone was importuned also to send aid to the starving Irish, and they were aided most royally. Equally, too, when the month of August, 1880, came on, and showed a mean-temperature higher than any August that had ever been chronicled by the Meteorological Society of Scotland before, who did not rejoice in the genial air, praise the splendour of the grain-crops by every roadside, and feel thankful to the Giver of all good? That, however, was after the event. The previous misfortunes and ruinations had occurred, and wrought their worst at their own times, because they were unlooked-for visitations. Why, therefore, had not the forecasts contained in the Edinburgh volume for 1872 been prepared for, and acted on in 1878 and 1879? Why had not agricultural science stepped in, and arranged, possibly, some scheme of rotation of crops suitable to a cold and wet year coming for certain twice, and an extra warm one once, in eleven years? For surely, those facts being known long beforehand, it should be within the powers of science and capital combined to ease off the pressure of climatic circumstances ever so untoward, or even disastrous, when they occur unexpectedly.

Anything, at all events, is better than the plan of the untutored savage (and equally of the ideal of a general peasant proprietary of the land), who lives merely from hand to mouth, thinking that "every successive year will be just like the last, only much more abundant." But in our highly-civilized country, fore-warned by eight long years should have been fore-armed. Why, then, was it not, in the melancholy period of 1878-9?

(*To be continued.*)

THE PERSEIDS.

Sir,—The radiant point of the Perseids, as observed this year at Bristol, was at 44° + 56°, from a mean of all the paths noted between August 6—13, and there was a sub-radiant at 45° + 60°. These positions agree with Major Tupman's observations on Aug. 9, when he recorded several meteors from a centre in higher declination than the normal radiant near η Persei. Mr. Corder gives 45° + 58° as the best position from 254 Perseids seen by him at Writtle, between July 29—Aug. 11, and Mr. Sawyer, at Boston, U.S.A., places the radiant at 44¾° + 56¼° from a bright stationary meteor, and a few short tracks observed on Aug. 9, during a watch of four hours, in which 54 Perseids were recorded.

At Bristol a considerable number of foreshortened meteors were registered, among them being the following:

	Time.	Mag.	Path. From α	Path. From δ	Path. To α	Path. To δ	Length.
1880.			°	°	°	°	°
Aug. 6	11 25	4	48	+ 53	54	+ 53½	4
7	...	2	54	+ 60	60	+ 60	3
„	12 50	3	46½	+ 52	48½	+ 50½	2
„	12 54	♂	52	+ 48½	55	+ 45	4
9	11 11	2	49	+ 58	52	+ 58	2
„	11 45	2	47	+ 49	48½	+ 47	3
„	11 52	4	49	+ 54	50½	+ 53	1½
10	11 15	3	51	+ 57	54	+ 57	1½
„	11 28	3	44	+ 59	44	+ 60	1
11	10 57	1	56	+ 60½	62	+ 62	3½
12	11 53	2	28	+ 55	23	+ 54	3½
„	12 7	3	59	+ 65	68	+ 68	4½

These meteors, with the exception of perhaps the first in the list, obviously belonged to the shower of Perseids. They all left bright streaks. But though I saw considerably more than 400 meteors during a series of observations between Aug 6—13, only a few of the paths were registered, as I was engaged in counting the horary numbers, and if this feature is to be reliably ascertained, the sole attention of the observer must be directed towards it. A meteor shower cannot be observed with anything like completeness unless two persons are engaged in the work, one registering the paths and peculiarities of the meteors, while the other watches the sky uninterruptedly, and counts the hourly numbers of conformable and non-conformable meteors.

Several of the minor showers of the August epoch have this year been observed with great distinctness. Mr. Sawyer saw 12 meteors on August 9, with a well-defined centre, near ξ Cassiopeiæ, at $5^\circ + 55^\circ$. At Bristol two conspicuous showers were noted at $30^\circ + 46^\circ$ and $46^\circ + 27^\circ$. The latter I have already pointed out (*Monthly Notices*, Vol. XL., p. 130) as a conspicuous display in the mornings of August 8—11. The meteors are very swift, with streaks, and unusually long tracks. Three of them, which I recently observed, were as follows:

	Time.		Mag.	From		To		Length.
				α	δ	α	δ	
1880.	h.	m.		°	°	°	°	°
Aug. 7	13	35	2	27	+ 52	342	+ 68	27
12	11	45	2	28½	+ 24	24	+ 23	5
13	12	15	2	13	+ 31½	2	+ 31	9

The two latter were somewhat foreshortened near the radiant.

On August 12, at 12h. 37m. I saw a meteor of exceptional length, and belonging probably to a new radiant just rising in Eridanus. It began at $63^\circ + 34^\circ$ and ended at $165^\circ + 76^\circ$ pursuing its course of 59° with moderate velocity, and leaving a very slender streak. On August 28, at about 11h. 30m. a meteor was seen with a path of little more than 1° from $123^\circ + 70\frac{1}{2}^\circ$ to $122^\circ + 69\frac{1}{2}^\circ$.

Ashley Down,
Bristol: Sept. 5.

I am, Sir, yours faithfully,
W. F. DENNING.

REVIEWS.

Aspect de la Planète Mars pendant l'opposition de 1879, *et observations de la tache rouge de Jupiter et des taches de Vénus.* Par M. F. Terby, Docteur en Sciences, à Louvain. Académie Royal de Belgique (Extrait des Bulletins, 2nd série, tome XLIX., No. 3; Mars, 1880). pp. 16.

Dr. Terby has been a constant observer of Mars since 1864. He has mostly used a telescope by Secretan, 3·5-in. aperture, having an excellent objective, and powers varying from 80 to 240. Occasionally, by permission of M. Houzeau, he has availed himself of the 6-in. equatorial of the Royal Observatory at Brussels. The observations on Mars are from September 28, 1879, to December 18, and are accompanied by twenty-three drawings. Those on the red spot on Jupiter are from November 9, 1879, to January 28, 1880, and are accompanied by seven tinted drawings. The observations on Venus, with ten drawings, were made in 1871 and 1873. They are introductory to a general study, which the author has undertaken, of all the spots on Venus which have been observed to our own days. They are notoriously very difficult to observe; nevertheless, after the fine observations of Bianchini, Cassini, and De Vico, their existence cannot be questioned. The old astronomers endeavoured to reduce the great brilliancy of the planet by diaphragms or by smoked glasses. Thus C. Huygens (1724) recommends for Venus and Mercury the slight smoking of the lens nearest the eye. Dr. Terby has often found the employment of diaphragms of various apertures before the objective to be of great service in observations of Jupiter, Mars, and Venus. His careful and persistent study of these planets, even with moderate means, has been very useful, and we are glad his attention continues to be directed to them.

Études et Lectures sur L'Astronomie. Par Camille Flammarion. Tome neuvième, accompagné de 32 figures astronomiques. Paris: Gauthier-Villars, 1880. pp. viii. 291. 2s 6d.

This volume is on the physical nature of the sun, its spots, eruptions, etc., and their connection with terrestrial magnetism, electricity, and auroras. Amongst the illustrations is a photoglyptic proof taken at the observatory of Meudon, of a part of the solar surface (Oct., 1877), enlarged three times from the original. The diameter of the disc being 3-ft., this proof, which is about 3-in. × 4-in., represents a portion of the surface about 1′ 20″ by 1′ 47″, or 35,000 by 47,000 miles. M. Flammarion remarks that it possesses a high interest, inasmuch as the human hand has had nothing to do in the production of this image, which is entirely due to the action of light. There is also an interesting diagram of the comparative curves of auroras, magnetic declination, and sun-spots, from 1778 to 1878, or one century; and another by Wolf, the comparison of the number of solar spots with the magnetic declination, from 1745 to 1879. The publication of the author's "Popular Astronomy" has delayed the appearance of this 9th vol. In the next M. Flammarion hopes to take up other correspondences between the sun and the earth. He observes, however, that "unfortunately the study of the heavens absorbs so entirely those devoted to it, and the earth revolves so fast, that truly the whole of life passes like a rapid voyage, in which one does not realise the tenth part of the designs that enter into the dreams of hope." The interest of this volume will be found not less than of any in the preceding series. It contains a number of observations and tables which are of value, irrespective of any special views or theories on still controverted points.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

SOME MEASURES OF THE SMALL STARS IN THE GROUP OF ε LYRÆ.

Very few measures of the faint stars between ε^1 and ε^2 Lyræ have been made. This is partly owing to the extreme faintness of many of the stars, and partly no doubt to the fact that no special interest attaches to them. The following measures are believed to be nearly all that exist. But in order that they may be readily available the stars must be described. The seven principal stars are well known and easy objects. They are: A B (ε^1), C D (ε^2), E the 9½ mag. star following them, F and G, the *debilissima* pair, F being the southern and preceding star. These were discovered by Sir William Herschel. Then in order of discovery come three stars H I J, to the west of F and G, detected by Lamont. Between the most northern of these and A B is Dawes's star K. A little north of G is Lassell's star L, and to the south-east of G is M, detected by Sadler and Ward in 1876. In the same year Mr. Pratt very carefully examined this group and raised the number of stars visible in it to 24, and a diagram showing them will be found in the *Observatory* for September.

The measures of some of these objects are as follows:

Lamont's in 1837 are angular positions.

C, D and F	339°	F and G	38°	G and E	91°
C, D and G	1°	F and H	71°	G and I	67°
C, D and H	131°	F and I	100°	H and I	171°
		F and E	70°	I and J	98°

A and J	44°	C and E	36°
C and A	352°	A and E	134°

Wilson and Seabroke in 18— made the following measures and estimations:

A, C	173°	Dist. 210″
A, E	135°	
A, F	180°	
A, G	165°	„ 110″
C, E	36°	„ 131″
C, F	338°	„ 72″
C, G	1°	„ 103″
E, F	250°	
E, G	268°	
F, G	38°	

In 1876 Mr. Pratt obtained the following:

C, A	350°	Dist. A, C	210″
C, F	337°	„ C, G	92
C, G	24°		

Mr. Burham also found time in 1878 to take a few measures of some of these stars. (See Flammarion's Catalogue, p. 166.)

	°	″
F, G	38·4	46·71
E, H	357·0	25·00
F, E	247·3	42·57

There is something wrong in the lettering of these stars; F, G is all right, but E, H and F, E present difficulties of identification, as an examination of the diagram above referred to will show.

Mr. Edward Crossley's Observatory, J. GLEDHILL.
Bermerside, Halifax.

RETURN OF AURORA.

The prevalence of a few fine solar spots of late may well have led observers to expect a recurrence of the attendant phenomenon.

After an interval of some years, during which no aurora worthy of mention has shown itself, a rather striking display began at 10h. 29m. on the evening of August 12. From a white band of light in the northern horizon five wide streamers suddenly rose, reaching an altitude of 35° to 40°. At a point due north there was an especially vivid one. For a few minutes an arc of greenish-white light continued in view in the NNE. horizon. The whole phenomenon was bounded by Capella in the NE., and by Arcturus in the west. The extreme brightness lasted only 12 minutes, after which the phenomenon gradually dwindled away. The sky was, otherwise, perfectly clear, a very unusual circumstance at the time of an aurora. Many meteors passed about this time, but none were observed to cross the aurora, so that there was no opportunity of noticing whether they increased in brightness in so doing. The Perseids were well observed this year on the nights of the 9th, 10th, 11th, half of those seen on the first two nights being = 1st magn.

Abbenhall Rectory, S. J. JOHNSON.
Gloucester: Aug. 14, 1880.

TRANSIT INSTRUMENT IN THE OBSERVATORY OF PADUA, September 22, 1829.

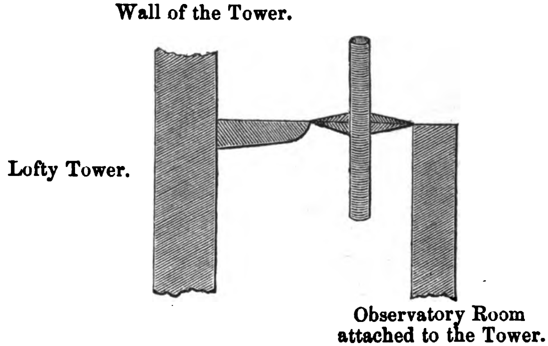
Sir,—You have lately published some interesting records of the state of astronomy in Italy for many years past.

Allow me to make a small contribution to your work, by a careful copy of a sketch of the mounting of the transit-instrument at Padua, which I drew on the spot in the year 1829.

I may mention that, at that time, the public clocks were regulated to Apparent Solar Time.

I am, Sir, your faithful servant,

Royal Observatory, Greenwich, G. B. AIRY.
London, S.E.: 1880, September 4.



LUNAR OBJECTS SUITABLE FOR OBSERVATION IN OCTOBER, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

In the *Astronomical Register* for August we called attention to a new series of observations of the spots and streaks of *Plato*, and solicited our readers to join in this work that greater accuracy may be attained. The observations have now been continued eight lunations, and a series of numbers obtained by the aid of three observers, which express the degree of visibility of fourteen spots, as in the following table :

Spot.	D. J.	F. M.	A. M.	I. J.
1	1·000	1·000	1·000	1·000
4	1·000	·937	1·000	·900*
3	1·000	·812	·888	·800*
22	1 000	·062		·400
14	·800	·250	·777	·700*
5	·800	·062	·222	·300*
16	·600	·062	·222	·300*
17	·200	·500	·777	·600*
7	·200	·062		
25		·125		
6		·125		·200
30		·062		·100
13				·200
19				·300

From these numbers, which are based upon 202 observations, may be selected the degrees of visibility of six spots, viz., Nos. 4 and 3 most frequently seen, Nos. 14 and 17 not so frequently seen, and Nos. 5 and 16 but rarely visible. The range of No. 4 equalled ·100, the curve furnished by the numbers reaching its maximum of 1·000 in the lunations of April and May. The range of No. 3 equalled ·076, the maximum of 888 being attained in the same lunations. The curve of Nos. 14 and 17 were much bolder, the degrees of visibility in the lunations of February and March of No. 14 was as low as ·250, maximum in April and May ·777 range ·527, which constituted the ascending branch, the descending branch being less steep, only ·077. Spot No. 17 commenced at ·500, attained a maximum of ·777 in April and May, same as No. 14, range ·277 of the ascending branch ; the curve fell in June and July to 600, so that the range of the descending branch was ·177. The spots most rarely seen were Nos. 5 and 16, both had an upward movement from ·062 to ·300, range ·238. The asterisks indicate the spots which have furnished curves. Observations of these spots, as well as others that may be seen, will be acceptable if sent to—

Corinium House, Romford Road, Stratford, E.

Books received.—Observations on Mount Etna. By Prof. S. P. Langley.—Elements of Astronomy. By R. S. Ball. London : Longmans, Green & Co. 1880 —Greenwich Observations : Photographic and Spectroscopic Results for 1878–9.—Observations on Mount Etna in 1878. By Prof. S. P. Langley.—Report of Observations made on Mount Hamilton. By S. W. Burnham. Chicago : 1880.—Preliminary Account of a Speculation and Practical Search for a Trans-Neptunian Planet. By D. P. Todd, M.A.

ASTRONOMICAL OCCURRENCES FOR OCTOBER, 1880.

DATE.		h. m.	Principal Occurrences.	Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m. Jupiter.
Fri	1		Sidereal Time at Mean Noon 12h. 42m. 22·72s.	1st Ec. D. 1st Oc. R.	13 28 44 15 49	12 14·2
Sat	2		Sun's Meridian Passage 10m. 49·53s. before Mean Noon	3rd Oc. R. 2nd Ec. D. 1st Sh. I. 1st Tr. I. 2nd Oc. R. 1st Sh. E. 1st Tr. E.	6 14 8 14 6 10 43 10 51 11 5 12 58 13 4	12 9·8
Sun	3	16 43	● New Moon	1st Ec. D. 1st Oc. R.	7 57 26 10 15	12 5·4
Mon	4	9 20	Conjunction of Moon and Mars 5° 38′ N. Conjunction of Moon and Mercury 4° 31′ N.	1st Sh. E. 1st Tr. E.	7 26 7 30	12 1·0
Tues	5	13	Conjunction of Moon and Venus 4° 31′ N.	3rd Sh. I. 3rd Tr. I.	17 31 17 50	11 56·5
Wed	6	22	Jupiter at opposition to the Sun			11 52·1
Thur	7			2nd Tr. I. 2nd Sh. I. 1st Tr. I. 1st Sh. I.	16 0 16 0 18 9 18 9	11 47·7
Fri	8			1st Oc. D. 1st Ec. R.	15 20 17 33 59	11 43·3
Sat	9			3rd Oc. D. 3rd Ec. R. 2nd Oc. D. 1st Tr. I. 1st Sh. I. 2nd Ec. R. 1st Tr. E. 1st Sh. E.	7 15 9 50 19 10 40 12 35 12 38 13 25 4 14 48 14 52	11 38·8
Sun	10	12 34 4 45 5 43 8 14 8 47	☽ Moon's First Quarter Occultation of B.A.C. 6524 (6½) Reappearance of ditto Occultation of B.A.C. 6561 (6½) Reappearance of ditto	1st Oc. D. 1st Ec. R.	9 46 12 2 42	11 34·4
Mon	11			2nd Sh. I. 1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E. 1st Tr. E. 1st Sh. E.	5 20 7 0 7 6 7 46 8 2 9 13 9 20	11 30·0
Tues	12			1st Ec. R.	6 31 34	11 25·6
Wed	13		Sidereal Time at Mean Noon 13h. 29m. 41·36s.			11 21·1
Thur	14		Sun's Meridian Passage 14m. 3·82s. before Mean Noon			11 16·7
Fri	15	6 23 7 27 12 55 14 2	Occultation of 16 Piscium (6) Reappearance of ditto Occultation of 19 Piscium (6) Reappearance of ditto	1st Oc. R.	17 3	11 12·3

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Sat	16	7 9 23	Near approach of 45 Piscium (6) Conjunction of Moon and Jupiter 7° 1′ S.	3rd Oc. D. 2nd Oc. D. 3rd Ec. R. 1st Tr. I. 1st Sh. I. 2nd Ec. R. 1st Tr. E.	10 30 12 53 13 51 21 14 18 14 33 16 0 14 16 31	11 7·9
Sun	17	16 26 22	○ Full Moon Conjunction of Moon and Saturn 7° 44′ S.	1st Oc. D. 1st Ec. R.	11 30 13 57 59	11 3·5
Mon	18	0	Saturn at opposition to the Sun	2nd Tr. I. 2nd Sh. I. 1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E. 1st Tr. E. 1st Sh. E.	7 25 7 59 8 44 9 1 10 2 10 40 10 57 11 15	10 59·1
Tues	19	17 3 17 56	Occultation of ζ Arietis (4½) Reappearance of ditto	1st Oc. D. 1st Ec. R.	5 56 8 26 53	10 54·7
Wed	20	10 22 11 31	Occultation of 32 Tauri (6) Reappearance of ditto	2nd Ec. R. 1st Tr. E. 1st Sh. E.	5 17 52 5 23 5 44	10 50·3
Thur	21	6 57 7 38	Occultation of τ Tauri (4½) Reappearance of ditto			10 45·9
Fri	22	8 59 9 52 10 52 11 59	Occultation of B.A.C. 1774 (6½) Reappearance of ditto Occultation of B.A.C. 1801 (6) Reappearance of ditto			10 41·5
Sat	23			3rd Oc. D. 2nd Oc. D. 1st Tr. I. 1st Sh. I.	13 46 15 7 16 2 16 27	10 37·1
Sun	24			1st Oc. D. 1st Ec. R.	13 14 15 53 22	10 32·7
Mon	25	19 0 8	☾ Moon's Last Quarter Conjunction of Mars and the Sun	2nd Tr. I. 1st Tr. I. 2nd Sh. I 1st Sh. I. 2nd Tr. E. 1st Tr. E. 1st Sh. E. 2nd Sh. E.	9 42 10 28 10 37 10 56 12 20 12 41 13 10 13 18	10 28·4
Tues	26			1st Oc. D. 1st Ec. R.	7 40 10 22 18	10 24·0
Wed	27			3rd Sh. I. 3rd Tr. E. 1st Tr. E. 1st Sh. E. 2nd Ec. R. 3rd Sh. E.	5 37 5 58 7 7 7 39 7 53 14 8 10	10 19·7
Thur	28			1st Ec. R.	4 51 8	10 15·3
Fri	29	15 43 16 32	Occultation of *e* Leonis (5) Reappearance of ditto			10 11·0
Sat	30					10 6·7
Sun	31			1st Oc. D.	15 0	10 2·3

THE PLANETS FOR OCTOBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	13 11 11	S. 7 24	4″·8	0 28·7
	9th	13 57 20	S. 12 50½	5″·0	0 43·3
	17th	14 42 13	S. 17 31	5″·2	0 56·6
	25th	15 25 38	S. 21 13	5″·8	1 8·4
Venus ...	1st	13 52 40	S. 11 7½	10″·6	1 10·1
	9th	14 30 28	S. 14 45½	11″·0	1 16·3
	17th	15 9 24	S. 18 0	11″·2	1 23·7
	25th	15 49 39	S. 20 45½	11″·4	1 32·4
Jupiter ...	1st	0 58 38	N. 4 29½	46″·8	12 14·2
	9th	0 54 40	N. 4 5	46″·8	11 38·8
	17th	0 50 45	N. 3 40½	46″·6	11 3·5
	25th	0 47 6	N. 3 19	46″·2	10 28·4
Saturn ...	1st	1 43 24	N. 7 44½	18″·0	12 58·9
	9th	1 41 8	N. 7 31	18″·0	12 25·2
	17th	1 38 46	N. 7 17	18″·0	11 51·4
	25th	1 36 23	N. 7 3½	18″·0	11 17·5
Neptune ...	3rd	2 47 24	N. 14 13½	...	13 54·9
	19th	2 45 51	N. 14 6	...	12 50·4

Mercury sets about a quarter of an hour after the sun at the beginning of the month, the interval increasing

Venus is too close to the sun to be well observed. She sets about three-quarters of an hour after the sun on the 1st, the interval gradually increasing.

Jupiter is very well situated for observation, rising soon after sunset at the beginning of the month, then earlier each day,

Saturn rises about 40 minutes after the sun on the 1st, and is visible throughout the night during the month.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Sept., 1880.

Burton, C. E.
Lewis, H. K.
Potter, Rev. T. J.

To Dec., 1880.

Elliott, R.
Schafarik, Prof. Dr.

To June, 1881.

Gooch, Miss

TO CORRESPONDENTS.

All communications should be addressed to the Editor.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The Astronomical Register.

No. 215. NOVEMBER. 1880.

THE ROCK-THERMOMETERS OF THE CALTON HILL, EDINBURGH.

By **Piazzi Smyth**, Astronomer-Royal for Scotland.

Part II.

(Continued from page 253.)

Alas! Alas! Alas! When everything was going on so favourably for this new order of long-shot meteorological prediction, in the earlier years following the Edinburgh publication of it in 1872, I was destined ultimately to be myself the needless ruination of it all. Had we been congratulating ourselves overmuch on the possession of such grand thermometers in the little and confined Edinburgh Observatory; or were we counting too fondly on the prospects that would open forth before the whole country in a few years more? I know not; but certain it is, that just at the moment when we were least expecting anything untoward, when the Government had appointed at last a special commission to look into the long-neglected affairs of their Royal Observatory of Edinburgh, and almost with an injunction that something was to be done for its effective development (agreeably with the long matured recommendations of their own Board of Visitors through many previous reporting years), just then it was that sudden destruction visited us, on the head too of the most precious and vital point we possessed.

In the dawn of the early morning, just two months after the sitting of that Government Commission, which was to have strengthened, perfected everything, or in September, 1876, a ferocious-looking, half-dressed sailor, from a Portuguese ship lying in Leith Docks, rushed out before any cf the inhabitants were about, coursed through all the length of Leith Walk (the "long-walls" which connect the modern Athens with its Piræus port), and at the head of that well-paved Appian way ascended the lonely Calton Hill. Arrived on the summit, he sought out a dilapidated crevice in the surrounding wall of the Royal Observatory, climbed over thereby into that confined enclosure, went straight up to the little house over the rock-thermometers, and with his great booted feet stove in its miniature door, and broke the whole of the glass tubes above ground in one mighty crash: putting thereby a forcible end, a death limit to that grand course of Edinburgh rock-thermometer observations which had been going on so steadily, and with continually advancing results, for no less than forty years.

The perpetrator of this transcending mischief escaped then, but was found later in the day committing further havoc in some other part of the city. So he was duly taken into custody, lodged in the police cells, and a few days after hauled up before the judge; when fragments of the once-existing earth thermometers were laid before that dignitary, as material witnesses of the crime committed, and we looked to see some terrible punishment awarded. But then learned medical evidence was adduced that the prisoner must be held mentally unaccountable for his actions; so a warrant was kindly made out in the usual form for his philanthropic detention in one of her Majesty's well-established and richly supported asylums for the insane.

From there soon afterwards the police lieutenant obligingly procured me a photograph of this, to the progress of thermometrical knowledge, most pernicious maniac; and to my astonishment I beheld a well-made, even handsome young man, under thirty, dressed in new, well-cut, broadcloth suit, with a high forehead, and, excepting only a rather overhanging brow, a generally good phrenological as well as facial development. A man evidently qualified, I should have thought, to be up and doing his part as a self-supporting member of the community: but who had now contrived to get the community to support him, in better dress, on better food, under a better roof than ever he had had as a sailor before the mast, and without any hard work or severe exposure: but at many, many times times the expense to this country of what it would have cost the Office of Works to have kept the boundary wall of the Edinburgh Royal Observatory in better repair.*

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* Lieut. Linton, of the Edinburgh Police, has lately (Oct. 1880) informed me that the man, John Stevens by name, was discharged from the Morningside Asylum, on the authority of the Portuguese Consul, on Feb. 17, 1877, and was then sent to Portugal; since which all trace of him has been lost.

Small consolation was that to the said Royal Observatory for the loss of those thermometers which could never be fully replaced. With the residual splinters of them of course no further observations could ever be made; and though new thermometers, if the Government should be very gracious, might, after a time,* be inserted in the old bore-hole in the porphyry rock, yet they could never be expected to have precisely the same zero points and value of scales as the older, and now vanished, instruments. The new ones, therefore, would have to be considered as a series by themselves; and who amongst us will be living, and how far on the road to a red republic may not this country under its present system of ministerial government have advanced, by the time that another series of forty years of observations shall have been collected ready for future computers.

Will the heroic reader ever excuse me if I confess that I felt quite overthrown by this unprecedented thermometrical calamity; whereby I could no longer contribute to the science of the world peculiar data on the progress of earth-surface temperature. I knew no more from this time of how that element was going on, than any of the rest of mankind; probably not so much; indeed, I began to credit everyone else with knowing vastly more about it. And so it came to pass, that when the season was approaching for bringing out the next volume of the Edinburgh Observatory work, and a continental physicist communicated to the public his researches on the dates of sun-spot minima, and asserted that the time for the then impending one was much earlier than what I had previously concluded,—of course I allowed at once that I must have been quite wrong, and he no doubt perfectly right; and I altered my previously published dates for the next cold, and next hot wave accordingly: giving out in the fourteenth volume of our observations, published in the end of 1877, that the said two opposing supra-annual visitations would arrive respectively just about a year sooner than I had given out in the thirteenth volume in 1872; and because the minimum of solar spots had been announced on better authority to be a year earlier also.

Oh! fatal mistake; oh! ever regrettable conclusion; for when the thus erroneously altered and hastened date for the cold wave came round, and was not cold,—that was quite enough for an indignant public, who enquired nothing further, but on that one and sole failure condemned the whole principle by which the forecast was supposed to have been arrived at: and would have it, that there was nothing whatever either of science or truth in connecting terrestrial meteorology with such things as sun spots.

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* A new set was inserted at Government expense in 1879.

But in the depths of the social ostracism which thence ensued, and the relapsed official neglect of the Royal Observatory, Edinburgh, which followed, I was enabled to look into the case with calmness and in remarkable quiet, endeavouring to find out what had been wrong. An ultimate error, no doubt, had been perpetrated, but that did not prove that everything which went before had been equally wrong and entirely baseless. I had only to unrol the large diagrams of the forty years of thermometrical observations again, to see that. So at last I plucked up courage, with faith enough to assert, that if the preliminary one of the two cold waves of terrestrial climate had not taken place in 1877·5, as latterly expected, it must have been because the minimum of solar spots did not then take place either, though it had been so confidently announced on the Continent; and on the strength of that announcement, and that alone, the error had been made by me, and introduced into the fourteenth volume of the Edinburgh Observatory's publications, to the prejudice of what had been already printed and published in the thirteenth.

But who shall bell the cat? Sun spots are such an intricate kind of phenomenon, so influenced for days together by something invisible to us under the photosphere of the sun, that steady and continued observation for a long period is necessary to decide the real and exact crowning epochs of the more or less 11-year cycles, and to distinguish them from other and smaller, or slower, or quicker, changes. What, therefore, was one individual without any particular preparation or apparatus likely to do, much less to establish, in contest with the regularly appointed and well-furnished exclusively solar observatories in various parts of Europe? Nothing in the way of inducing anyone else to believe in any very different conclusions on the generality of solar phenomena, I felt sure; for the work of those solar observatories is well done and worthily credited in every civilised land. But special attention to some one point might elicit, even from humble means, something worthy. I therefore confined my attention entirely to the date of the minimum, by such observations as I could compass at my own house, and rather by means of the physiognomy of the spots than anything else.

These observations, however—begun, too, more on my Wife's promptings than my own—were soon accompanied by extremely reassuring results; for they not only proved that the minimum of sun spots had not occurred in 1877·5, but that it was not altogether reached even in 1878·5.

The method of observing was extremely simple. By means merely of a heliostat on a long plank,—originally provided for solar spectroscopy, and now pushed out of a southern window of an upper chamber in the astronomer's dwelling-house,—a ray of sun-light was introduced at any time of the day, and made to form an image of the sun nearly a foot in diameter,* on a screen capable of revolving in position vertically, and showing accurately the N. and S., preceding and following, points of the sun's circumference. The minute physiognomy of every spot of tolerable size could then be examined both most easily and very often, an essential requisite, and it proved enough; for after weary, weary watching of sun spots through 1878, and the beginning of 1879, and seeing all that time only small spots evidently under difficulties of formation, there burst forth such an altered character of them on October 18, 1879, such a thundering proof that the forces of a new cycle in real and positive action had then abundantly begun, that I wrote of it the very same day to *Nature* journal in London, under the title of "Sun spots in earnest"; and I congratulated everyone I met that day in Edinburgh on the late cold season of 1878 and 1879, having then well passed and received its *quietus*, because the solar forces were now thoroughly recuperated: and though, during the next six months, chiefly to be devoted to the southern hemisphere, yet certain to come to us in the north afterwards, or in the course of 1880.

Now, although this particular and energetic display of large spots alluded to occurred only, as intimated above, in the autumn of 1879, yet the more moderate increase leading up to it, or the turning of the scale between the decrease of the last eleven-year cycle and the increase of the present one, must have been much earlier, probably in the beginning of 1879. Putting, therefore, that date into the sun-spot curves of a fourteenth Edinburgh volume, in place of the erroneous 1877·5 borrowed from abroad, behold the original predictions of cold wave and hot wave in a thirteenth Edinburgh volume, founded on our own thermometrical observations alone, were abundantly justified—*first*, by the weather experiences of the whole country in 1878-9; and *next*, by the more exact observations of the sun-spots made at Greenwich. The Greenwich confirmation has, indeed, been very notable, for after my letter came out in *Nature* on "Sun spots in earnest," a communication appeared from Mr. Christie, Chief Assistant at Greenwich, to almost exactly the same double effect

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* The ray was passed through an objective of 7-feet focal length, and then virtually extended by being further passed on by a deep plano-concave lens, originally employed in flattening the field of a photographic camera. But this latter lens has since then been replaced by a special achromatic concave prepared for me by Messrs. T. Cooke and Sons, of York.

—viz., of the excessive increase of sun spots on October 18, 1879, and the smaller increase marking a new eleven-year cycle, in the beginning of 1879: and when the Astronomer Royal printed his report to the Board of Greenwich Visitors in June, 1880, he repeated in effect both statements.

Nor have the confirmations stopped there, for on March 17 I had the good fortune to witness the first aurora of the new solar cycle, and wrote of it to *Nature* as being such; whereon there came next week the *quasi* confirmation from Greenwich that a magnetic storm had occurred there the same night; and several weeks afterwards came the announcement from Canada of an aurora having been observed there, also on March 17, also after a long *interregnum* of auroral displays, and so like the Edinburgh aurora "that the same description," said Colonel Bulger, the Canadian observer, "would serve for both."

Since that date the strength of the solar rays has gone on increasing, also the number, size, variety, and activity of formation of sun spots from month to month; while the auroras that next broke forth on August 11 and 12, as well as the magnetic storms of the same dates, have shown an intensity without any parallel for many years past.* Everything, therefore, is now abundantly proving that we are at present well advanced on the new eleven-year cycle of sun spots and auroras, with all their consequences; that the spot minimum was passed in 1878·9, or 1879·0; that the cold wave felt by the whole country, to a loss of 150 millions in agriculture, coincided therewith; that the hot wave followed 1·7 year therefrom† (culminating in August and September, 1880), that we are now in the beginning of descent to the next cold wave, but which will probably not be severe; and then there will be several ordinary years before the next grand hot wave comes upon us, immediately preceded and followed, as usual, by its attendant cold waves.

"When, then, exactly will that next heat-wave come?" do you ask? Why 1·7 year after the next minimum of sun spots.

"And pray, when will that take place?"

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* See *Nature*, No. 564, vol. XXII. for Aug. 19, 1880; also No. 572, for Oct. 14, with the interesting account of a similar and simultaneous magnetic storm at Melbourne, Australia.

† The improvement of temperature was felt in Scotland suddenly and markedly in the end of December, 1879, and in January and February, 1880; then followed several months which were unexpectedly cold; but by reason apparently of another effect of extra heat in the sun's rays loosening the ice-fields in polar regions, and sending them down in such extra quantity to lower latitudes that, even in June, vessels to and from the United States were numerously reported as endangered, and sometimes injured by them.

That is precisely what no man can say in the present imperfect knowledge which all modern science labours under as to the causation, laws, and theory of sun-spot formation and manifestation.

I have been probing the authorities at Greenwich deeply, I fear they may think rather troublesomely, on this very point: but I applied to them because they are observing the sun spots officially, and with every aid they desire, optical, photographical, spectroscopic, and mensurational; and they take account not only of the number and size of the dark spots, but of the bright spots as well; and note the rapidity of formation, the parallels of solar latitude and the circles of longitude, where they appear, and the paths they pursue. In short, the Greenwich final determination of the lately passed minimum is, I believe, the best determined date of such a solar phenomenon that has ever been observed anywhere and at any time. Not even M. Schwabe's own observations can compare with the completeness and perfection of the last and newest results from Greenwich; while as to the imposing array lately published on the Continent of all the dates of all the minima, and all the maxima of all the sun-spot cycles which have occurred during 150 years past, or for more than 100 years previous to M. Schwabe's discovery of their being any periodicity in them,—I do not believe that one of them can be trusted within many years.

But even Greenwich, with all its newly organised power of *observing* a sun-spot minimum accurately, is only in its infancy in *predicting* when the next similar minimum will take place.

"If the average is to prevail," says an unpublished letter from thence, "1890 will be the year of the next solar spot minimum."

True, no doubt! But what assurance is there that "the average" will prevail? None that I know of; while the alleged variations of past minima amount to nearly two years less, or two years more, *i.e.*, may vary over four years; this being the received table of only the last seven minima.

Dates.	Differences.	Dates.	Differences.
1810·6	12·7	1843·5	12·5
1823·3	10·6	1856·0	11·2
1833·9	9·6	1867·2	11·8
		1879·0	

Now, in that case, with the sun spot intervals varying possibly close to three years in successive cycles, and the greatest authority in this country, or the whole world, confessing from his castle of Greenwich, that he has not the slightest idea whether the now commenced cycle will prove a long, a short, or an average one in length—the epoch of the next hot wave with its attendant cold waves must be equally uncertain, or by three years; and that is far too wide a limit to enable a prediction to be of any practical use to farmers, or to the State either, to enable them to prevent, or even decrease, another lamentable loss of 150 millions of money, as in 1879.

What, then, should be done to improve the outlook for the future, and reduce the uncertainty of the predicted epoch from 3 to, say, 0.3 years? Why, of course observations of sun spots ought to be multiplied, varied and refined, and mathematical theorists engaged to look into the question from time to time. That, at least, has been the course of improvement in every older branch of astronomical science; and no reason has been shown yet why the sun spots, and more comprehensively the cycles of the sun's radiant energy, should not eventually yield to its searching qualities also.

But there is a lion in the way! Not a genuine one indeed, but a very loud-voiced imitation, concocted by a small knot of writers, mostly anonymous, in the weekly and daily press; who have taken to abuse sun-spot observations, and to warning Government, and the people too, against sun-spot observers, as though they were a mere designing set of pickpockets on the national purse. For a late example see it actually on page 141 of *The Astronomical Register* for the present year. In fact, everything said there against Government aid, in this latter day, being afforded towards establishing a special observatory for enabling sun-spot phenomena to be at some future day accurately predicted, would have been of equal force in Charles the Second's reign against founding the Greenwich Observatory for the purpose of observing the moon, in order that her place in the sky might ultimately be predicted with greater exactness than of old, for the use of sailors on long voyages; as everyone knows is now so grandly accomplished at last.

Some of the unknown critics have, indeed, argued that there is a difference between the two cases, because the site, chiefly recommended for a sun-spot observatory, is not in Great Britain, but in India. Yet why should it not be there? Has not Great Britain had to pay for famines in India, depending on dislocations of the usually regular tropical weather; and have not these dislocations been traced in the epochs of their occurrence to the sun-spot cycles; and was not the last Madras famine actually told of, *before it occurred*, by Mr. Norman Pogson, the Government Astronomer there, solely on sun-spot data, combined, though imperfectly, with accounts of earlier Madras famines?

But even if there were no such famine years in India, and if it was solely in the interests of British and Irish agriculture that the enquiry was to be made, which is likely to be the better situation for a Solar Observatory—London, where clouds, fogs, smoke, and a low winter altitude of the sun, cause long periods to pass without the solar disc being ever visible, or tropical India where, according to popular impressions, every morning brings on a fine hay-making day, and the inhabitants have to be protected from too much sunshine.

See, too, what a pointed contribution of positive experience has been furnished by a further episode in the history of those particularly remarkable sun spots of October 18, 1879, which caused me to write to *Nature* in such jubilant confidence of a new sun-spot cycle being then not only commenced, but grandly under way. I recently received two *fasciculi* of sun-spot observations from abroad; one by Professor Tacchini in Southern Italy, the other from Dr. Bredichin in Moscow; the former situated between London and India—one may say on the road to India—the latter north of both, and therefore naturally less favourable even than London: and this was the solid printed result.

Professor Tacchini had so magnificent a view of those most crucial spots, and was so much impressed by them, that he had introduced a special picture plate of their appearance to him on October 19; and awful physical existences they did look, opening out of, and whirling round in, a sea of supernal fire floating above an unfathomable abyss of nameless, driest heat. But Dr. Bredichin, poor man! had neither seen, nor known anything about them. From October 3, when there was, to him, a group of very "little spots with faculæ not far from the W.S.W. limb," to October 26, when all he could find to note was "two little faculæ, round, near W. limb"; he had had no solar observations at all. And yet he is a most admirable and active observer of every astronomical phenomenon which the geographical position of his observatory allows him to see at all.

Surely, then, there can be no doubt as to where, in her vast territorial possessions encircling the whole earth, Great Britain's chief sun spot, and cosmical-meteorological, observatory of the future ought to be established; and I can speak so very freely, and I trust fairly, of the whole matter, because now quite disinterested personally. My woeful mistake of altering my first prediction and adopting at second-hand for the last sun spot minimum's date, the quantity 1877·5, has, together with the Portuguese reputed madman's destruction of all our late forty-year-old rock thermometers, closed my career in that line for ever.

Never again can I speak before the public, of any further of those most strange supra-annual cycles of temperature, over-riding, yet commingling with, the ordinary summers and winters so happily secured to us by an ordinance of God. This great country of ours must in future be served in that line by some one of her sons who has never made a mistake; never allowed himself to be misled by uncertain authority, and least of all by foreign authority. I find it, therefore, high time now to bid a long adieu to all the more exciting hopes and wholesome fears, engrossing labours and pure delights, inspiriting contests and eventual triumphs of essaying the cosmical in meteorology by appealing to the sun itself. I am content, moreover, to leave it all with this last remark; which may, too, be not without some advantage, even to certain most unforgiving critics of sun-spot researches, in these latter days of openly proposed extirpation of rich, titled landlords (in Ireland) by the short, summary process of shooting them, in order that the peasants may acquire and subdivide the land among themselves alone: namely—

If it was written of old in the sacred Book of Revelation that "the stars in their courses fought against Sisera"; so now it seems to be found indelibly formulated in the Book of Nature,—that the Sun, in his never ceasing cycles of radiant heat, light, magnetism, and spots, with their necessarily accompanying tenth or twelfth bad agricultural year, "*fights against the Irish Land League*"; and will ever prevent a universal peasant proprietorship of the country succeeding to anything so certainly as, periodical starvation and general distress. While if it be further bragged by demagogues, that all the poorer population of the south and west of Ireland have now become so firmly, universally, and unalterably convinced, as if by inspiration, that their future prosperity depends on their destroying out landlordism from amongst them, though it alone has root and capital sufficient to tide over an eleven-year cycle's monster of a bad season, the crushing answer which thermometrical science endorses, while it tends to explain, is "Quem Deus vult perdere, prius dementat."

15, Royal Terrace, Edinburgh: P. S.
October 15, 1880.

OBSERVATIONS MADE DURING TOTAL SOLAR ECLIPSES.

(Continued from page 226.)

XLIV. "In order to facilitate the comparison of the drawings of the corona . . . . all the woodcuts, with the exception of a few which would have been too large for the page, have, in pursuance of a suggestion of the Astronomer Royal, been made on one scale, and the coronas have been turned round, or oriented, so as to bring the sun's axis vertical upon the page with the north pole uppermost. The data are always given from which the position of the sun's axis has been determined." The scale for the woodcuts is 1·25 inches for the moon's diameter; but in order to give all the details visible on some of the photographs, the lithographic plates from the photographs of the eclipses of 1851, 1860, and 1869, and the plate from the Jerez photograph of 1870, as well as the steel plates from Lord Lindsay's and Colonel Tennant's photographs of the eclipse of 1871, have been made on double the scale of the woodcuts, or 2·5 inches for the mean diameter. The large plates showing the structure visible on the 1871 photographs, are on eight times the scale of the woodcuts, or ten inches for the moon's diameter. "In casting the eye over the pictures of the corona . . . . the reader may possibly, in the first instance, be more struck by the dissimilarity of the drawings than by their likeness. But when we remember the short time during which totality lasts, the various matters to which the attention of the observer may be directed, the different methods which he may select for representing what he sees, and the difficulties which even a skilled artist encounters in attempting to represent an object like the corona,* it is perhaps remarkable that there should be as much similarity between the various drawings and descriptions of the corona as will, on a careful comparison, be found to exist. Mr. Ranyard in the first place directs attention to the structure of the corona in the eclipse of December 12, 1871. "It is evident that the corona changes greatly from eclipse to eclipse; but owing to the excellence of the photographs taken during that eclipse, we are probably better acquainted with the corona of that date than with any other." In the photographs of this eclipse are two marked rifts, one to the north and the other to the south. The line joining the base of these rifts divides the corona into two halves, which are roughly symmetrical . . . . the line about which the corona appears to be symmetrical does not coincide with the sun's axis." There are also curving forms and rays, showing contrary flexure, which it is remarked appear to indicate the existence of an atmosphere with currents carrying the matter of which the structures are composed with different velocities at different altitudes. Tree-like structures also seem to indicate the spreading out within a resisting atmosphere of matter rising from below. The forms of the structures do not seem to afford evidence of a repulsive force similar to that which drives the matter of a comet's tail away from the sun; and in passing from the lower to the higher parts of the corona, it will be noticed that there is no abrupt change in the brightness of the corona, such as might be expected to correspond to a region of dissociation, or to any other abrupt change in the physical state of the coronal matter. Most of the pictures of the coronas which have been observed during the various eclipses exhibit, when oriented, a general symmetry of the corona with respect to the sun's axis. In most instances there is a difference of some 5° or 10° between the position of the axis of symmetry and the sun's axis. Mr. Ranyard observes that the "appearance of symmetry must be greatly interfered with as the rays are seen in projection by an observer on the earth. Rays inclined to or from the observer would have their apparent inclination with respect to the sun's axis materially altered, and long rays in the nearer and further parts of the corona would frequently appear to spring from a part of the sun's limb corresponding to a different zone of heliographic latitude. In examining the drawings . . . . these considerations must continually be kept in mind."

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* "These difficulties will be more readily appreciated when we remember the discrepancies which exist between the drawings of a well-known telescopic object, like the nebula in Orion, an object which may be observed for many hours at a time, and with which the draughtsman can, if he wishes it, repeatedly compare his drawing."

"There is evidence tending to show that in many of the coronas which have been observed, there have been four great groups of synclinal structure situated more or less symmetrically with respect to the sun's axis—indicating that within two broad zones of heliographic latitude the coronal structures incline together . . . . the heliographic latitude and the breadth of the synclinal zones has differed considerably on the different occasions when evidence of their existence has been obtained." The evidence afforded by different eclipses respecting the presence or otherwise of these zones of synclinal structure is presented; and for the examination of the question whether or not there is a connection between the sun-spot period and the heliographic latitude of the synclinal zones, the epochs of maximum and minimum sun-spot development given by Dr. R. Wolf in Vol. XLIII. of the *Memoirs*, are made use of. Designating lines drawn from the sun's centre, so as to divide the synclinal groups symmetrically as the axes of the synclinal groups, these axes are mostly found to have made angles of about 45° with the sun's axis; but in the eclipse of 1871, they made angles from 30° to 40° with the sun's axis.

The earliest attempt made to photograph the corona was by Prof. Majocchi (Milan) in 1842, which was unsuccessful; but in

1851, Berkowski, Busch, and Wichmann, at Königsberg took a daguerreotype picture. The daguerreotype plate has been preserved, and is now at the Strasburg Observatory, in the possession of Professor Winnecke, who lent it to an exhibition of scientific instruments at South Kensington, in 1876. Plate II. at the end of the present volume, was made from a steel engraving published in Vol. XXVI. of the *Königsberger Astronomische Beobachtungen,* but it very fairly represents the corona as shown upon the daguerreotype plate. In 1860 Mr. Warren De la Rue made two photographs at totality, but only slight traces of the corona were obtained. This was at Rivabellosa. At Desierto de Las Palmas, Padre Secchi and Professor Monserat obtained five photographs. Plate III. in this volume was made by combining the details visible in photographic copies (on paper) of four of these negatives. In 1868 the photographs taken at Aden and Guntoor, though excellent as pictures of the prominences, were not sufficiently exposed to show more than traces of the lowest parts of the corona. In 1869 many photographs were taken. "The photographs of the corona taken at Shelbyville show two broad rifts in the neighbourhood of the sun's poles, and four great groups of structure in the N.W., S.W., S.E., and N.E. quadrants. Plate IV. was made from paper copies of the Shelbyville photograph, which were sent to England in 1870 and 1871. . . . the original negative is now in the possession of Prof. Pickering, at Harvard College Observatory." Mr. Ranyard adds, "From a cursory examination of this negative, which I made during a visit to Harvard in 1878, it was evident that a great deal of structure was recognisable in the original negative which is not shown in the steel plate published in Vol. VI. of the 'Annals of the Harvard College Observatory.' A good deal of structure is also to be seen in some of the other negatives taken at Shelbyville during totality, which were only exposed for a few seconds. Accurate drawings of the structure to be seen in these photographs, under suitable illumination, are much to be desired." The broad rifts in the polar regions indicated by the Shelbyville photograph do not seem to have been noticed by any of the observers (except possibly Simon Newcomb, and Cutts). Mr. Ranyard says they were probably partly filled up by faint coronal matter, and that they indicate that the synclinal zones were more depressed towards the equatorial regions than in the corona of 1871. On the other hand, it seems that the synclinal zones occupied higher heliographic latitudes than in the corona of 1868. Of the eclipse of 1870 we have two photographs, Plates V. and VI. Plate V. from a glass positive taken directly from the negative obtained at Jerez by Mr. Willard; and Plate VI. engraved from a drawing made from the original negative taken at Syracuse by Mr. Brothers. "This eclipse took place at a period when solar activity, as evidenced by sun spots, was exceptionally great. . . . The corona was of great extent, both in the equatorial regions and in the regions of the sun's poles; but it was probably more homogeneous in its lower parts than the coronas of 1869 and 1871, when the number and development of sun spots was considerably less. The coronal structure, as well as being less striking as compared with the background on which it was observed, seems to have been somewhat less symmetrically arranged with respect to the sun's axis than in other coronas observed at recent eclipses." "The photograph taken by Mr. Brothers at Syracuse . . . shows the corona extending to a much greater distance from the sun's limb, more especially in the polar regions, than the photographs taken in 1871." "The great rift in the S.E. quadrant seems to have formed a very striking feature of this corona, and to have been either drawn or described by at least ten of the observers whose observations are given in the following chapter." Mr. Brothers remarks that "the photographs taken during the eclipse prove that the light of the corona is very actinic, and several photographs of this beautiful phenomenon may be taken during the time of totality. Judging from the effect of the coronal light on the sensitive plate, I should estimate its intensity near the moon's limb as quite equal to that of the full moon." The negatives of Mr. Brothers are now in the possession of the Royal Astronomical Society. The mezzotint engraving for Plate No. VI. has been made from a very conscientious drawing by Mr. W. H. Wesley of the detail visible upon Syracuse negative No. 5, as seen under various illuminations. Mr. Wesley's drawing is now deposited in the library of the Society. We are told that "although at first sight there does not appear to be much detail in this drawing, its production occupied Mr. Wesley nearly three months."

Excellent photographs were taken during the eclipse of Dec. 12, 1871. "Five, of which the definition is very sharp, were taken at Baikul, and six on the same scale at Dodabetta; and although the dark moon is represented by a circle only 3-10ths of an inch in diameter, and the whole field of the corona would be more than covered by a sixpence, an examination of the photographs under suitable illumination yields very satisfactory evidence as to all the structure represented in plates VII. to XVII. It would be impossible, from an examination of a single negative, to determine whether any small marking has its origin in some minute impurity in the collodion, or whether it represents a structure in the corona; but by a comparison of the different negatives the photographic flaws can with certainty be eliminated. And a careful examination proves that all the coronal details which can be made out in the Dodabetta photographs can also be traced with certainty in the photographs of the Baikul series, in which the definition is rather sharper." And in a note it is stated, "Photographs showing a great deal of coronal structure were taken during the eclipse of July, 1878, but the details visible in them have not at present been drawn and catalogued. A cursory examinaton of the negatives proves that there was a great deal of difference between the structure as well as the general form of the corona on the two occasions. In the 1878 photographs the structures are more easily recognised, and they appear to have been somewhat coarser, and probably also brighter, as compared with the background in which they were projected than in the corona of December 12, 1871. The tree-like forms, as well as the synclinal groups, which were a marked feature of the corona of 1871, appear to be absent from the corona of July, 1878."

"Plate IX. shows very well the irradiation from the prominences extending over the disc of the dark moon—a phenomenon which is common to all corona photographs, and proves the great actinic intensity of the light of the prominences as compared with the coronal matter adjacent to other parts of the moon's limb." "In order to exhibit the evidence which exists for each of the coronal structures given in the plates a catalogue has been prepared containing a list of the negatives upon which each detail can be distinguished. In this work Mr. Ranyard says, "I had the good fortune to be aided by a most accurate and conscientious artist (Mr. Wesley, the present Assistant-Secretary of the Royal Astronomical Society), for whose laborious perseverance in the task I cannot be too thankful. Before any detail of coronal structure was entered in the catalogue it was drawn by each of us separately; and unless our drawings agreed, and we recognised the structure upon three of the negatives it was not entered in the catalogue." These details of the catalogue extend from p. 658 to 686. These coronal pictures remind one in their general shape of the head gear, feathers, etc., of the caps of North American Indians. "Plates X. to XVII. show, on a scale of ten inches to the sun's diameter, the structures which have been entered in the catalogue. The distinctness of the coronal details has been intentionally exaggerated, for it would have been impossible to reproduce by lithography the delicate differences of opacity to be seen in the negatives; and if it had been possible, the plates would not have been so serviceable as diagrams to compare with the descriptions of the structures given in the catalogue. Plates VII. and VIII. more nearly represent the appearance of the photographs, though in these the structures have necessarily been made somewhat more conspicuous than they appear on the photographs, even as seen under the most favourable circumstances."

In addition to the photographs taken at Baikul and Dodabetta, two photographs of the corona were taken in Java, the structures which can be recognised in them are referred to and described in the catalogue; and there is no evidence of change having taken place in the form of the coronal structures, as far as they can be recognised, during the period of an hour and four minutes between the times of mid-totality at Baikul and the Java station. On which Mr. Ranyard remarks in a note, "If we take into account the small scale of the photographs, and the enormous magnitude of the coronal structures, it will be evident that the matter of the corona may have been moving with velocities which were great compared with the velocity of the earth in its orbit, without any sensible change, such as would be recognisable in the photographs, being brought about in a period of one hour." "The longest rays of the corona, as shown upon the Baikul photographs, do not reach to a height of 27′ above the sun's limb, while in the Syracuse photograph of the corona of 1870 the corona is traceable to a height of at least 40′ above the sun's limb. In the Dodabetta photographs the extent of the corona is not quite so great as in the Baikul series; and in the copies of the photographs taken in Java the corona does not extend to a height of much more than 15′ above the sun's limb. But according to the evidence of Capt. Herschel and Capt. Tupman, the corona must have been visible to a much greater distance above the sun's limb. Capt. Herschel describes the longer rays seen without a telescope as extending to "one and a-half diameters" from the moon's limb; and Capt. Tupman, observing with a telescope, describes a narrow rift in the N.W. region of the corona as "distinctly traceable to a distance of 45′ or 50′ from the moon's limb." . . . "The groups of rays are not shown in many of the drawings, and the woodcuts chiefly serve to show how inferior the evidence to be derived from drawings is, compared with that to be derived from photographs, especially when an attempt was made to make more than one drawing of the corona during totality." In regard to changes in appearance, as seen in perspective, Mr. Ranyard observes, "A straight ray cannot by projection be seen as curved, nor could a curve of single curvature be projected into a curve exhibiting contrary flexure. And though a ray which is not normal to the sun's surface may, as seen in perspective, appear radial to the sun's limb, a ray which does not appear radial to the sun's limb cannot be normal to the sun's surface. We may, therefore, assert that some of the coronal rays were certainly not normal to the sun's surface. And there appears to be ample evidence that some of the coronal structures, curved in different directions at different altitudes, apparently indicating the existence of an atmosphere in the coronal regions, with currents carrying the matter of which the structures were composed in different directions, or with different velocities at different altitudes." We may add concerning the plates, that Plate VIII. is engraved from a drawing made from the original negative, taken at Baikul by Mr. Davis, Lord Lindsay's assistant. VIII. is from the original negative taken at Dodabetta by Mr. J. B. N. Hennessey and Capt. Waterhouse, under the superintendence of Col. Tennant. IX. is a drawing showing the photographic details of negative No. I. of the Dodabetta series, black representing white.

Eclipse of 1874, April 16th. "We do not possess any photographs of the corona visible during this eclipse, but the drawing given by Mr. Stone in his report published in Vol. XLII., proves the corona to have been very extensive. In Mr. Wright's drawing the rays extend to a distance of more than three solar diameters from the sun's limb, and Mr. Stone traced the 1474 line with a spectroscope to more than a degree from the sun's centre. The corona seems to have been of the same type as that observed during the eclipse of 1868. The drawings made by Mr. Wright and Miss Alice Hall were very similar to Capt. Bullock's 1868 drawing; they represent the corona with four great groups of rays, the axes of which make angles considerably greater than 45° with the sun's axis. . . . None of the observers seemed to have noticed any structures within the field of the corona. . . . It would seem probable that such as may have existed were not as striking compared with the background on which they were seen projected, as in the coronas of 1860 and 1871; and as far as such negative evidence goes, there would appear to be a similarity between the corona visible during this eclipse and that observed during the eclipse of 1870, though the general outline of the coronas visible on the two occasions differed greatly." The relative number of sun-spots given by Dr. Wolf for April, 1874, about corresponds with that given for the period of the eclipse of 1868, and is only a little more than one-third as great as that given for the period of the eclipse of 1870. The corona visible during the eclipse of 1875, April 6, seems to have been similar in type to that observed during the eclipse of the previous year. An outline woodcut is given from copies of photographs in the *Philosophical Transactions* for 1878, which show four groups of synclinal structure, situated nearly symmetrically with respect to the sun's axis. Their axes make angles of more than 45° with the sun's axis. "From our knowledge with respect to the rotation of the photosphere, derived from observations of sun-spots, it appears that different heliographic latitudes have different rotation periods, and it seems not improbable that a similar law would hold with regard to the corona." The solar activity, as indicated by sun-spots, was not half as great as at the period of the eclipse of the previous year. Some account of the eclipse of 1878, July 29th, appeared in the *Astronomical Register*, Vol. XVI., pp. 241—247, 294—299; XVII., pp. 185—188, and in the *Observatory*, Vol. II., pp. 162—163, 187—191, 195—197, 234—235, 262—266, which our readers may like to turn back to for comparison with previous eclipses.

We shall conclude this notice of the corona chapter by an extract from the address of Prof. Langley at the Saratoga meeting of the American Association for the Advancement of Science. August, 1879: "A review of our past studies of the corona is a review of the solar eclipses during the past 20 years, for it is a fact unparalleled in the sciences of observation that the opportunities for this knowledge last only minutes, and are separated by intervals of years. Till 1860 it was uncertain whether the protuberances belonged to the sun or moon, but in that year the then newly-applied photographic method made it nearly certain that they were parts of the former, and previous surmises that they were extensions of an envelope everywhere surrounding the sun, were confirmed. In 1868 some traces of the corona were first photographed. [Prof. Langley has overlooked the daguerreotype of the corona of 1851.] The spectroscope was used upon the prominences, their gaseous nature was proved, and nine of the chromospheric lines were determined; and nearly together Messrs. Janssen and Lockyer made the discovery that these lines could be seen without an eclipse; 1869 brought that eclipse which traversed our own territory, and in this the distinctive coronal line was first observed by Young and by Harkness [Prof. Langley has overlooked the observation by Rayet, August, 1868, who called the line E] while in this, and yet more in the eclipses of 1870 and 1871, we obtained better photographs of the corona, and greatly increased our knowledge of its apparent structure. . . . Of recent coronal studies I have only to speak of the opportunity afforded by the eclipse of last year in our own western territory. Observed as it was in the pure air of the Rocky Mountains, we found an immense and hitherto almost unsuspected extension of the corona in the direction of the solar equator, such as to make it increasingly probable that the outer corona and zodiacal light are different appearances with a common origin. The physical constitution of the inner corona seemed to be modified by the weakness or absence of a former constituent, and perhaps we may say that some additional knowledge was gained as to its telescopic structure and its absolute light, while the polariscopic evidence was contradictory.

In the light of our latest knowledge, what, then, is the corona? We do not know. We have literally had but about 20 minutes in the last 20 years to look at it, and from that brief study it remains every way problematical. The extent of this vast solar appendage is unknown, its constitution is unknown, its function is unknown, and it is still uncertain whether we can devise any means for its study which will free us from this dependence upon momentary glimpses. Our only hope, since the most powerful telescope is useless in our lower atmosphere, seems to be to transport our observatory to some mountain height, like that of Etna or the elevated table-lands of Colorado. There, even, we cannot be sure of seeing it without an eclipse ; but there, if anywhere, ingenuity will be hopefully employed in an endeavour to remove the difficulties which bar the way. After spending some weeks this year myself upon Mount Etna, on which the new solar observatory is to be built, I can testify to the excellence of such a station, and yet, when we have sites equally good, I cannot but regret that it should be left to others to first enter such a promising field."

Allusion is frequently made to integrating and analysing spectroscopes, and, as some may not understand the difference, they may be glad of the explanation given in Mr. Proctor's "*Sun*," etc., 3rd edition, p. 139. "A spectroscope may do one of two things. Either it may form the spectrum corresponding to light reaching the slit from a considerable range (as, for instance, the whole space occupied by the sun, and perhaps the illuminated sky for a width of two or three degrees); or else it may give the spectrum corresponding only to a narrow line of light. In one case the spectrum is produced by the spreading out, as it were, of multiplied images of the illuminated slit, the illumination of the slit being derived from a wide area of light. In the other the spectrum is formed of multiplied images of the line of light under examination. A spectroscope serving the former purpose may be called (as Professor Young, of America, suggests) an *integrating spectroscope;* one of the latter kind an *analysing spectroscope.*"

Many besides professed astronomers are now living, ladies as well as gentlemen, who may see their names among the hundreds recorded in this eclipse volume. The author has made the most of every one's contribution, and left, as it seems to us, no room for impeachment of his fairness and impartiality. Next to the privilege of witnessing a total eclipse and contributing somewhat to the faithful record of its phenomena, comes that of having such a work as the present, entertaining to read, and invaluable for study. Its preparation must have been toilsome, and we think Mr. Ranyard must have been glad when he put the colophon to his writing, and yet more when he saw it complete in type in the goodly, well-printed quarto, of which we have endeavoured to present an outline. As we have said, it is a work of no mere ephemeral interest, but a κτῆμα ἐς 'αεί—a perpetual possession; for such a mass of well-arranged and well-sifted observations, so finely and copiously illustrated, will never lose its value, whatever fresh discoveries may be made in future years. And the Royal Astronomical Society, under whose auspices it has been produced, may be congratulated on the addition of such a volume to its memoirs, which, as it was eagerly hoped for, we cannot doubt will be proportionately prized.

THE NOVEMBER LEONIDS AND ANDROMEDES.

It is to be hoped that the two chief meteoric displays of November will be attentively observed this year. In the case of the Leonids there will be some interference from moonlight, our satellite being full on the evening of the 16th, and visible nearly all night on the 13th, when the meteors usually attain their maximum abundance. But observations should be sustained over the period from the 11th to the 15th, inclusive, in the morning hours. On the 13th the moon sets at 3h. 30m. a.m., and on the 14th at 4h. 43m. a.m. After these times until about 5h. 30m. a.m. (when twilight will have become too strong) the eastern sky should be persistently watched for the swift, streak-leaving meteors, diverging from Leo. There will be little difficulty in identifying them from the contemporary meteors belonging to other showers in the same region of the sky, for the direction of motion will infallibly guide the eye back to the radiant point between the stars ν and ϵ of Leo. The streaks almost invariably projected on their courses are an important aid in fixing this position. If the observer holds a perfectly straight rod at arm's length, and in conformity with their directions, carrying the eye back along the rod, the point of divergence in Leo will be distinctly apparent, and this method of recording the flights tends to greater accuracy than in cases where the eye solely is relied upon to estimate the direction. The radiant point of this notable shower has been exactly defined from previous observations, and the more valuable feature now to be ascertained is the visible intensity of the shower. In every case the observer should record the time occupied in watching, the total number of meteors seen, and the proportion conforming to the radiant in Leo. Though the display in 1879 was not in any way comparable with what has been witnessed near the time of its periodical maxima, it yet formed an active shower, and the importance of tracing its fluctuations year by year cannot be over-estimated. Its annual apparitions lead us to the obvious inference that it forms, like the

Perseids of August, a continuous ring, though the particles of the latter stream are undoubtedly dispersed more thickly along the entire orbit than the Leonids, which seem mainly confined to the region of their derivative comet. But that there are detached meteor-groups following the same orbit, and hence belonging to the same system is evident from observations in past years, when, though the comet has been separated from the earth by a distance nearly as great as that of Uranus, the shower has returned as the earth passed the node. And there is no reason to question the physical identity of the showers observed in different years, the date and radiant being absolutely coincident, and the individual meteors exhibiting special peculiarities of motion and appearance precisely agreeing both in the major and minor displays of the series. The radiant does not rise until about 11 p.m., so that it is useless to look for these meteors early in the night; indeed, the shower to be witnessed under favourable auspices, must be seen in the morning hours, when the radiant has attained a fair elevation. This point, as deduced last year by the several observers who were fortunate enough to witness its reappearance, was situated in an exactly similar position to that noted in 1866, when Prof. Herschel placed it at 149° + 23°, from an average of many good observations. In 1879 the results were:—

	°	°		°	°
H. Corder	149	+ 23	Rev. S. J. Perry	147½	+ 23
E. F. Sawyer	151	+ 22	W. F. Denning	148	+ 23

The Andromedes due on about the 27th, may be observed under more favourable circumstances than the Leonids. The sky will be free from moonlight, and, moreover, the radiant close to ν Andromedæ, being visible all night, the shower may be looked for at any hour convenient to the observer between the 25th and 29th, for the time of its apparition is open to some uncertainty. Last year a considerable display was expected, but no meteors were seen in the brilliantly moon-lit skies at the end of November, and if the shower recurred at all, it must have been one of extremely meagre character.

Since 1872 very few of these meteors have been observed. A series of morning watches in 1876, Nov. 25 to 28, revealed no traces of the shower. In 1877, Nov. 25, there were slight indications of its return, for at Bristol during a watch of 1½ hour (5h. 30m. to 7h.) 7 slow meteors were recorded from the radiant at 24° + 45°. In 1878 Mr. Corder, at Chelmsford, saw 17 meteors between 5h. 30m. and 9h. 30m., of which 6 were Andromedes, including two of the first magnitude near the radiant which appeared to be at about 23° + 44°. This is close to Prof. Herschel's position at 25° + 43° for the maximum in 1872. The meteors are characterized by extreme slowness of motion, and they seldom, if ever, leave the phosphorescent streaks which form so persistent a feature of the Leonids, Perseids, and other swift meteors proceeding from radiants near the apex of the earth's way. They appear also to be smaller generally than the members of the other chief streams, but in point of numbers, the display of 1872 must compare favourably with any similar exhibition of modern times, and though a bright shower cannot be certainly predicted to occur, observers should be constantly on the alert during the last week of November, for it must be remembered that negative results will have their value as proving this stream to have, for the present, quitted the region of the earth's path. There is no reason to believe that the meteorolites are distributed over the whole cometary orbit; indeed, the total failure to recover the shower for several years subsequently to its exceptional appearance in 1872, showed that this system is much condensed, and confined—at least, in its grander displays—to the special epoch when the earth intersects the node at a time the comet is near. Even these conditions may fail, as in 1879, and the importance of future observations of this remarkable shower cannot be doubted. Careful records of its apparently intermittent returns must finally prove the key to its fuller elucidation.

Ashley Down, Bristol: W. F. DENNING.
October 8, 1880.

THE LATE MR. LASSELL.

In Mr. Lassell, who died on Tuesday, Oct. 5, in his 82nd year, another bright luminary is extinguished in the astronomical world, and his friends mourn the loss of one of rare worth. His name will ever be remembered for the splendid reflecting telescopes which he constructed, their admirable mounting on a plan of his own, and the discoveries made with them. Commencing about 1820 with mirrors 7-in. diameter, he afterwards made a very fine one of 9-in., and in 1844 one of 2-feet aperture and 20-feet focal length. With this he discovered, in 1847, the satellite of Neptune, and in 1848, on the same night with Professor Bond, of America, Hyperion, an eighth satellite of Saturn. In 1851 he discovered Umbriel and Ariel, satellites of Uranus; and in 1852, having taken this telescope to Malta, made with it there a series of observations, which, as Sir John Herschel remarked, rendered his residence there "a remarkable epoch in the history of astronomical observations." It was likewise at Malta that his grand reflector, 4-feet ap. and 37-feet focus, equatorially mounted, was erected in 1861. The memoirs of the R. A. S., Vol. XXXVI., contain an account of the four years' work done with this instrument—observations of the satellite of Neptune, and the satellites of Uranus; of nebulæ and planets, and a catalogue of 600 new nebulæ. After his return from Malta, Mr. Lassell purchased an estate near Maidenhead, where he erected his equatorial telescope of 2 feet ap. It was there that he constructed an improved form of polishing machine (Trans. R. Society, 1874). Amongst other honours conferred on Mr. Lassell—and we may say equally reflected in such a case as his on the sources whence they were derived—were Fellowship of the R. A. S. in 1839; its gold medal in 1849; its Presidentship in 1870; Fellowship of the Royal Society, 1849, and one of its gold medals in 1858.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE DOUBLE STAR OΣ. 496.

This object was discovered by OΣ, in 1851, but as earlyas 1845 he wrote, "il parait qu'il y a une étoile très faible à la distance d'environ 2″ de A.": in 1853·91 it could not be seen by the discoverer. It does not appear that either Dawes or Dembowski ever saw it, though both observers frequently examined this group of stars. In 1879 Mr. Sadler requested me to look for it, and on several nights it was sought for in vain by both Mr. Crossley and myself. Not much importance was attached to this failure as the star was low and far to the N.W. A similar result, however, attended the attempts this year when the star was in the N.E. It is but fair, however, to add that on none of these occasions was the sky at all good. Mr. Sadler also was unsuccessful, and the general impression among observers was that the star was not double. A communication from Mr. Burnham, just received, sets the matter at rest. OΣ. is quite right; the star is really double, and was seen as such on the 18th of August last by Mr. Burnham. As I have not yet received his measures nothing can now be said about motion, binarity, etc. The following notes will enable observers readily to turn to and examine this difficult object. OΣ. 496 is a member of a group of eight stars, two of which are known as P. XXIII. 100. 101. The place for 1880 is R.A. 23h. 24m. Dec. + 57° 53.′ The magnitude of A is 5·4, and OΣ.'s companion is of the 10th magnitude. The star B. is also double the magnitude of the companion, being 7·4; Dawes discovered this: it was also independently detected by OΣ.

A few observations will decide whether the companion of OΣ. 496 is often invisible owing to variability, or simply to its faintness and proximity to the brighter star.

Mr. Crossley's Observatory: J. GLEDHILL.
Halifax.

9 *PERSEI.*

This star has a minute attendant, which has escaped observation heretofore. My measures are as follows:

	°	″	
P =	162·5	11·66	1880·59
	161·5	11·60	·61
	162·0	11·49	·63
Mean =	162·0	11·58	1880·61

The companion I have estimated to be a little brighter than 12·5 mag. of Struve's scale.

Chicago: August 29th. S. W. BURNHAM.

Σ. 2789.

The slow angular motion shown by the measures of O. Σ. is confirmed by two recent observations of my own:

		°		″		
Σ	P =	226·7	D =	0·78	1834·96	6 nights.
O. Σ.		228·8		0·85	1844·97	3 „
β		230·3		0·85	1880·58	2 „

After measuring this I found a new pair in the vicinity, which may be the star measured by O. Σ. and Secchi for Σ. 2879. The first measure by O. Σ. of Σ. 2879, was recorded as 128°·8, which he was led to suppose by later measures was an error in reading for 288°·8. He suggests, however, the possibility of there being another double star nearer Σ. 2879, since an observation by Secchi, credited to Σ. 2879 agrees closely with the uncorrected measure referred to.

Assuming these measures to belong to the new pair, they stand as follows:

		°		″		
O. Σ.	P =	128·8	D =	0·91	1840·61	1 night.
Se.		130·5		0·5	1856·93	1 „
β		122·3		0·65	1880·61	3 „

This pair is 5m. 46s. preceding Σ. 2879, and 18′ south, and is the only new double star I could find in this neighbourhood. tI is very much more difficult than the Struve pair, from the inequality of the components. Argelander gives this as a ninth magnitude star, but I have estimated the components as 8·3 and 9·7 magnitudes. Taking the considerable distance of this star from Σ. 2879, and its difficulty with a large aperture, I think it is hardly likely to be the one referred to above, but future measures will aid in determining this, since it should have a slow direct motion.

Chicago. S. W. BURNHAM.

HARTWIG'S COMET.

Sir,—I looked for Hartwig's comet on the 2nd, but owing to the errors in the first notice could not find it, though I swept for a great distance from its supposed position. After receiving a correct account I easily found it on the 8th, at 8h. 50m.; with my 4¼ in. equatorial it had a tail about 20′ long. It was just visible to the naked eye, as a star. It was not a very clear night.

I had the best view of its spectrum on the 11th, at 10h. 25m., and the 13th, at 7h. 20m., in spite of the moonlight. I made no measurements, but the spectrum seemed to be the usual one of comets; it consisted of three bright bands, of which the middle one was much the brightest, while the most refrangible (which was about twice as far off it as the least refrangible) was not easily visible. I used Browning's "miniature spectroscope" on the telescope.

Sunderland: Oct. 15, 1880. T. W. BACKHOUSE.

ε LYRÆ.

In the *Register* for October there is an omission, and an erratum. The former is supplied by the date 1871-2, in the line beginning "Wilson and Seabroke." The erratum is in the account of Mr. Burnham's measures; a diagram just received from that gentleman enables me to give the correct letters to the stars measured by him. FC are the *debilissimæ* 38°·4, 46″·71; EH are the two stars preceding F, and forming with it an isosceles triangle, 357°·0, 25°·00; FE are the southern star of the *debilissimæ*, and the star at the south-western angle of the triangle just named, 247°·3, 42″·57.

It may also be added that Lamont independently detected the three faint stars preceding F, (that is, H, I, J), but Dawes had seen them in 1833.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax. J. GLEDHILL.

TABLE OF NEW MOONS.

From 1850 to 1883.

By the Rev. W. J. B. Richards, D.D., F.R.A.S.

The following Table has been compiled from the volumes of the *Nautical Almanac.* It will be found useful for finding the age of the moon at any given date, and will facilitate the tabulating of observations according to the moon's age—which is desirable in the study of certain phenomena, such as the light streaks in the Mare Crisium, or the appearances on the floors of Plato and Archimedes.

1850.

Jan. ...	12	23	19
Feb. ...	11	18	29
Mar. ...	13	11	17
April ...	12	0	47
May ...	11	11	9
June ...	9	19	20
July ...	9	2	27
August	7	9	34
Sept. ...	5	17	28
Oct. ...	5	2	56
Nov. ...	3	14	40
Dec. ...	3	5	16

1851.

Jan. ...	1	22	43
„ ...	31	18	2
Mar. ...	2	13	15
April ...	1	6	33
„ ...	30	21	2
May ...	30	8	47
June ...	28	18	25
July ...	28	2	40
August	26	10	20
Sept. ...	24	18	12
Oct. ...	24	3	10
Nov. ...	22	14	6
Dec. ...	22	3	34

1852.

Jan. ...	20	19	27
Feb. ...	19	12	54
Mar. ...	20	6	43
April ...	18	23	45
May ...	18	15	15
June ..	17	4	47
July ..	16	16	15
August	15	1	58
Sept. .	13	10	38
Oct. ...	12	19	14
Nov. ...	11	4	41
Dec. ...	10	15	32

1853.

Jan. ...	9	3	53
Feb. ...	7	17	34
Mar. ...	9	8	19
April ...	7	23	57
May ...	7	16	6
June ...	6	8	3
July ...	5	22	54
August	4	12	6
Sept. ...	2	23	42
Oct. ...	2	10	18
„ ...	31	20	39
Nov. ...	30	7	13
Dec. ...	29	18	6

1854.

Jan. ...	28	5	12
Feb. ...	26	16	39
Mar. ...	28	4	52
April ...	26	18	14
May ...	26	8	47
June ...	25	0	2
July ...	24	15	16
August	23	6	0
Sept. ...	21	20	3
Oct. ...	21	9	25
Nov. ...	19	22	2
Dec. ...	19	9	47

1855.

Jan. ...	17	20	37
Feb. ...	16	6	48
Mar. ...	17	16	45
April ...	16	3	5
May ...	15	14	13
June ...	14	2	29
July ..	13	16	1
August	12	6	52
Sept. ...	10	22	52
Oct. ...	10	15	24
Nov. ...	9	7	31
Dec. ...	8	22	18

1856.

Jan. ...	7	11	17
Feb. ...	5	22	36
Mar. ...	6	8	39
April ...	4	17	53
May ...	4	2	42
June ...	2	11	37
July ...	1	21	30
„ ...	31	9	8
August	29	23	14
Sept. ...	28	15	48
Oct. ...	28	9	54
Nov. ...	27	4	1
Dec. ...	26	20	45

1857.

Jan. ...	25	11	26
Feb. ...	23	23	58
Mar. ...	25	19	28
April ...	23	19	14
May ...	23	2	48
June ...	21	10	3
July ...	20	18	12
August	19	4	26
Sept. ...	17	17	33
Oct. ...	17	9	38
Nov. ...	16	3	54
Dec. ...	15	23	1

1858.

Jan. ...	14	17	32
Feb. ...	13	10	13
Mar. ...	15	0	12
April ...	13	11	15
May ...	12	19	48
June ...	11	2	46
July ...	10	9	24
August	8	16	54
Sept. ...	7	2	15
Oct. ...	6	14	7
Nov. ...	5	4	48
Dec. ...	4	22	10

1859.			
Jan. ...	3	17	26
Feb. ...	2	13	4
Mar. ...	4	7	11
April ...	2	22	18
May ...	2	10	4
„ ...	31	19	10
June ...	30	2	41
July ...	29	9	44
August	27	17	14
Sept. ...	26	1	56
Oct. ...	25	12	32
Nov. ...	24	1	43
Dec. ...	23	17	47

1860.			
Jan. ...	22	12	17
Feb. ..	21	7	39
Mar. ...	22	1	56
April ...	20	17	45
May ...	20	6	46
June ...	18	17	23
July ...	18	2	19
August	16	10	19
Sept. ...	14	18	9
Oct. ...	14	2	39
Nov. ...	12	12	36
Dec. ...	12	0	48

1861.			
Jan. ...	10	15	27
Feb. ...	9	8	5
Mar. ...	11	1	38
April ...	9	18	56
May ...	9	11	8
June ...	8	1	38
July ...	7	14	12
August	6	0	54
Sept. ...	4	10	12
Oct. ...	3	18	56
Nov. ...	2	4	3
Dec. ...	1	14	17
„ ...	31	1	54

1862.			
Jan. ...	29	14	50
Feb. ...	28	4	49
Mar. ...	29	19	45
April ...	28	11	27
May ...	28	3	26
June ...	26	18	54
July ...	26	9	5
August	24	21	40
Sept. ...	23	8	58
Oct. ...	22	19	37
Nov. ...	21	6	15
Dec. ...	20	17	4

1863.			
Jan. ...	19	4	2
Feb. ...	17	15	6
Mar. ...	19	2	37
April ...	17	15	5
May ...	17	4	49
June ...	15	19	36
July ...	15	10	54
August	14	2	3
Sept. ...	12	16	42
Oct. ...	12	6	42
Nov. ...	10	19	59
Dec. ...	10	8	24

1864.			
Jan. ...	8	19	46
Feb. ...	7	6	10
Mar. ...	7	15	59
April ...	6	1	49
May ...	5	12	14
June ...	3	23	40
July ...	3	12	24
August	2	2	34
„	31	18	8
Sept. ...	30	10	43
Oct. ...	30	3	28
Nov. ...	28	19	17
Dec. ...	28	9	22

1865.			
Jan. ...	26	21	30
Feb. ...	25	8	3
Mar. ...	26	17	28
April ...	25	2	13
May ...	24	10	49
June ...	22	19	57
July ...	22	6	29
August	20	19	17
Sept. ...	19	10	46
Oct. ...	19	4	27
Nov. ...	17	23	0
Dec. ...	17	16	45

1866.			
Jan. ...	16	8	37
Feb. ...	14	22	13
Mar. ...	16	9	37
April ..	14	19	3
May ...	14	2	58
June ...	12	10	7
July ...	11	17	35
August	10	2	36
Sept. ...	8	14	14
Oct. ...	8	4	58
Nov. ...	6	22	25
Dec. ...	6	17	25

1867.			
Jan. ...	5	12	30
Feb. ...	4	6	16
Mar. ...	5	21	38
April ...	4	10	4
May ...	3	19	40
June ...	2	3	12
July ...	1	9	48
„ ...	30	16	43
August	29	1	5
Sept. ...	27	11	42
Oct. ...	27	1	3
Nov. ...	25	17	11
Dec. ...	25	11	39

1868.			
Jan ...	24	7	18
Feb. ...	23	2	20
Mar. ...	23	18	59
April ...	22	8	20
May ...	21	18	36
June ...	20	2	45
July	19	9	56
August	17	17	11
Sept. ...	16	1	19
Oct. ...	15	11	1
Nov. ...	13	22	55
Dec. ...	13	13	33

1869.			
Jan. ...	12	6	53
Feb. ...	11	1	54
Mar. ...	12	20	46
April ...	11	13	47
May ...	11	4	7
June ...	9	15	52
July ...	8	1	38
August	7	10	8
Sept. ...	5	18	7
Oct. ...	5	2	19
Nov. ...	3	11	35
Dec. ...	2	21	41

1870.			
Jan. ...	1	12	5
„ ...	31	3	41
Mar. ...	1	20	40
„ ...	31	13	58
April ...	30	6	37
May ...	29	21	57
June ...	28	11	33
July ...	27	23	18
August	26	9	26
Sept. ...	24	18	34
Oct. ...	24	3	36
Nov. ...	22	13	21
Dec. ...	22	0	18

1871.

Jan.	20	12	32
Feb.	19	1	49
Mar.	20	16	0
April	19	7	3
May	18	22	45
June	17	14	29
July	17	5	27
August	15	16	1
Sept.	14	7	9
Oct.	13	18	19
Nov.	12	5	8
Dec.	11	16	1

1872.

Jan.	10	2	57
Feb.	8	13	51
Mar.	9	0	53
April	7	12	31
May	6	1	18
June	4	15	23
July	5	6	24
August	3	21	45
Sept.	2	12	53
Oct.	2	3	30
„	31	17	28
Nov.	30	6	34
Dec.	29	18	36

1873.

Jan.	28	5	27
Feb.	26	15	22
Mar.	28	0	54
April	26	10	42
May	25	21	20
June	24	9	12
July	23	22	33
August	22	13	30
Sept.	21	5	50
Oct.	20	22	55
Nov.	19	15	36
Dec.	19	6	49

1874.

Jan.	17	20	0
Feb.	16	7	14
Mar.	17	17	1
April	16	1	52
May	15	10	16
June	13	18	52
July	13	4	27
August	11	15	59
Sept.	10	6	16
Oct.	9	23	1
Nov.	8	17	33
Dec.	8	11	6

1875.

Jan.	7	5	8
Feb.	5	19	54
Mar.	7	8	20
April	5	18	35
May	5	3	3
June	3	10	20
July	2	17	24
August	1	1	27
„	30	11	41
Sept.	29	0	55
Oct.	28	17	12
Nov.	27	11	44
Dec.	27	7	4

1876.

Jan.	26	1	41
Feb.	24	18	20
Mar.	25	8	12
April	23	19	3
May	23	3	25
June	21	10	17
July	20	16	53
August	19	0	26
Sept.	17	9	54
Oct.	16	21	57
Nov.	15	12	48
Dec.	15	6	14

1877.

Jan.	14	1	28
Feb.	12	20	59
Mar.	14	14	54
April	13	5	48
May	12	17	29
June	11	2	32
July	10	10	6
August	8	17	17
Sept.	7	1	0
Oct.	6	9	58
Nov.	4	20	48
Dec.	4	10	4

1878.

Jan.	3	2	3
Feb.	1	20	17
Mar.	3	15	18
April	2	9	14
May	2	0	50
„	31	13	48
June	30	0	31
July	29	9	40
August	27	18	0
Sept.	26	2	10
Oct.	25	10	58
Nov.	23	21	11
Dec.	23	9	24

1879.

Jan.	21	23	51
Feb.	20	16	3
Mar.	22	9	4
April	21	1	56
May	20	17	50
June	19	8	20
July	18	21	6
August	17	8	11
Sept.	15	17	57
Oct.	15	3	9
Nov.	13	12	38
Dec.	12	23	4

1880.

Jan.	11	10	40
Feb.	9	23	17
Mar.	10	12	47
April	9	3	7
May	8	18	16
June	7	9	55
July	7	1	21
August	5	15	48
Sept.	4	4	52
Oct.	3	16	43
Nov.	2	3	55
Dec.	1	14	56
„	31	1	56

1881.

Jan.	29	12	48
Feb.	27	23	32
Mar.	29	10	32
April	27	22	24
May	27	11	36
June	26	2	4
July	25	17	19
August	24	8	45
Sept.	22	23	55
Oct.	22	14	31
Nov.	21	4	21
Dec.	20	17	7

1882.

Jan.	19	4	35
Feb.	17	14	50
Mar.	19	0	17
April	17	9	38
May	16	19	33
June	15	6	33
July	14	19	1
August	13	9	10
Sept.	12	0	59
Oct.	11	18	1
Nov.	10	11	20
Dec.	10	3	38

1883.				May ...	6	9	58	Sept. ...	30	17	54
Jan. ...	8	17	59	June ...	4	18	13	Oct. ...	30	11	57
Feb. ...	7	6	10	July ...	4	3	4	Nov. ...	29	6	54
Mar. ...	8	16	31	August	2	13	26	Dec. ...	29	1	0
April ...	7	1	36	Sept. ...	1	2	14				

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN NOVEMBER, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Two objects, apparently new to observers, have been seen lately on the moon's surface. The Rev. C. Gaudibert announces in letter 17,989 in the *English Mechanic*, Oct. 22, p. 157, col. 3, that on Oct. 14, 1880, at 9 p.m., he saw a double crater in a valley N.W. of Fra Mauro, which he had no recollection of having seen before. On referring to a sketch in his observation-book of the region made on Jan. 2, 1879, he found that no crater had been noticed in the locality on the date of the sketch. On the same evening, Oct. 14, at 10.30 p.m., Mr. Denning, of Bristol, saw on the floor of Plato a spot nearly S. of the conspicuous spot No. 17, which is given on the sketch made on that evening a little distance from the W., and within the light area of the sector. This spot he saw again on the evening of the 20th, but shows it on the border of the sector, and says in his letter, "I saw the small spot again S. of No. 17 which I had detected on Oct. 14, 10.30, but it lies not directly S. of 17, but slightly S.E., and, like 17, is situated on or near the border of the sector. Mr. Denning also remarks that on the 20th he saw a *very large number* of light specks scattered over the floor.

With regard to these probably new objects the double crater of M. Gaudibert may be classed with the two celebrated cases of Linné and Hyginus N, the idea of the manifestation of change in both cases being based on the *non-recollection* by the observers, Schmidt and Klein, of any phenomena of the kind having been seen by them before. In the case of the spot on Plato, although on the published diagram of the spots on Plato Report of the British Association, 1872, p. 24, 35 are shown, in the table facing p. 63 of the Report for 1871, the degrees of visibility of 36 are given, on p. 60 of the same Report it is mentioned that the number of spots actually observed between April, 1869, and April, 1871, amounted to 37. It is, therefore, uncertain if the spot S.E. of No. 17 had been seen during the interval between April, 1869, and April, 1871, and to regard it as new would be premature, unless future observations show that it may be classed with spots having a higher degree of visibility than it evidently possessed at the epoch of the publication of the Lunar Map Circular VIII.

On October, 1880, Mr. Denning gives a spot nearly midway between spots 22 and 13, but not in a direct line; it is really removed from such a line a little distance to the east; it is not identified.

Corinium House, Romford Road,
Stratford, Oct. 23, 1880.

Erratum on page 237, for the note at the bottom of the page, substitute the following: "Piazzi, through whom the first new hour of the century saw kindling in the sky a star unseen before."

ASTRONOMICAL OCCURRENCES FOR NOVEMBER, 1880.

DATE.		h. m.	Principal Occurrences.	Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m. Jupiter.
Mon	1		Sidereal Time at Mean Noon 14h. 44m. 35·89s.	2nd Tr. I. 1st Tr. I. 1st Sh. I. 2nd Sh. I. 1st Tr. E. 2nd Tr. E. 1st Sh. E.	12 0 12 13 12 51 13 16 14 26 14 39 15 4	9 58·0
Tues	2	3 54 2	● New Moon Conjunction of Moon and Mars 4° 20′ N. Sun's Meridian Passage 16m. 19·62s. before Mean Noon	1st Oc. D. 1st Ec. R.	9 26 12 17 50	9 53·7
Wed	3	20	Conjunction of Moon and Mercury 0° 49′ S.	2nd Oc. D. 1st Tr. I. 3rd Tr. I. 1st Sh. I. 1st Tr. E. 3rd Tr. E. 1st Sh. E. 3rd Sh. I. 2nd Ec. R. 3rd Sh. E.	6 30 6 39 6 54 7 19 8 52 9 21 9 33 9 40 10 28 43 12 11	9 49·4
Thur	4	8 10	Conjunction of Moon and Venus 0° 17′ N. Conjunction of Neptune and Sun	1st Ec. R.	6 46 42	9 45·1
Fri	5			2nd Sh. E.	5 16	9 40·8
Sat	6	7 10 7 31	Occultation of ν^1 Sagittarii (5) Reappearance of ditto			9 36·6
Sun	7	6 35	Near approach of 57 Sagittarii (5½)			9 32·3
Mon	8	20 19	☽ Moon's First Quarter	1st Tr. I. 2nd Tr. I. 1st Sh. I.	13 59 14 20 14 46	9 28·1
Tues	9	8 56 9 16	Occultation of c^2 Capricorni (6) Reappearance of ditto	1st Oc. D. 1st Ec. R.	11 12 14 13 28	9 23·8
Wed	10	8 39 9 39	Occultation of κ Aquarii (5) Reappearance of ditto Saturn's Ring : Major axis=44″·63 Minor axis=10″·43	1st Tr. I. 1st Oc. D. 1st Sh. I. 3rd Tr. I. 1st Tr. E. 1st Sh. E. 3rd Tr. E. 2nd Ec. R. 3rd Sh. I.	8 25 8 47 9 14 10 18 10 38 11 28 12 48 13 4 21 13 42	9 19·6
Thur	11	8 40 9 46 9 13	Occultation of κ Piscium (4½) Reappearance of ditto Near approach of 9 Piscium (6)	1st Oc. D. 1st Ec. R.	5 39 6 42 21	9 15·4
Fri	12	14 56	Occultation of 45 Piscium (6)	1st Tr. E. 2nd Sh. I. 1st Sh. E. 2nd Tr. E. 2nd Sh. E.	5 5 5 14 5 57 6 10 7 55	9 11·2
Sat	13	1	Conjunction of Moon and Jupiter 7° 7′ S.			9 7·0
Sun	14	2	Conjunction of Moon and Saturn 7° 51′ S. Sidereal Time at Mean Noon 15h. 35m. 51·11s.	3rd Ec. R.	5 57 10	9 2·8

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Mon	15		Illuminated portion of disc of Venus=0″·847			8 58·6
			Illuminated portion of disc of Mars=0·999			
Tues	16	8 39	○ Full Moon	1st Oc. D.	13 0	8 54·4
			Sun's Meridian Passage 14m. 58·63s. before Mean Noon			
Wed	17	6 55	Occultation of υ^1 Tauri (4½)	1st Tr. I.	10 12	8 50·3
		7 51	Reappearance of ditto	2nd Oc. D.	11 7	
		7 24	Occultation of υ^2 Tauri (6)	1st Sh. I.	11 10	
		8 27	Reappearance of ditto	1st Tr. E.	12 26	
		16 48	Near approach of τ Tauri (4½)	1st Sh. E.	13 24	
				3rd Tr. I.	13 48	
Thur	18	18 47	Occultation of B.A.C. 1774 (6½)	1st Oc. D.	7 27	8 46·1
		19 7	Reappearance of ditto	1st Ec. R.	10 38 6	
Fri	19	6 55	Near approach of B.A.C. 1970 (6½)	1st Sh. I.	5 39	8 42·0
		8 52	Occultation of η Geminorum (3½)	2nd Tr. I.	5 54	
		9 57	Reappearance of ditto	1st Tr. E.	6 53	
		13 39	Occultation of μ Geminorum (3)	1st Sh. E.	7 53	
		14 43	Reappearance of ditto	2nd Sh. I.	7 53	
				2nd Tr. E.	8 34	
				2nd Sh. E.	10 33	
Sat	20	8 16	Occultation of ζ^2 Geminorum (4)	1st Ec. R.	5 7 7	8 37·9
		9 14	Reappearance of ditto			
Sun	21	12 38	Occultation of 3 Cancri (6)	2nd Ec. R.	4 58 4	8 33·8
		13 38	Reappearance of ditto	3rd Oc. R.	5 57	
				3rd Ec. D.	7 43 23	
				3rd Ec. R.	9 58 24	
Mon	22					8 29·7
Tues	23	11 7	Occultation of *h* Leonis (6)			8 25·6
		12 8	Reappearance of ditto			
		7	Inferior conjunction of Mercury and Sun			
Wed	24	14 5	☾ Moon's Last Quarter	1st Tr. I.	12 0	8 21·6
				1st Sh. I.	13 5	
				2nd Oc. D.	13 29	
Thur	25			1st Oc. D.	9 16	8 17·5
				1st Ec. R.	12 33 55	
Fri	26			1st Tr. I.	6 27	8 13·5
				1st Sh. I.	7 34	
				2nd Tr. I.	8 19	
				1st Tr. E.	8 41	
				1st Sh. E.	9 48	
				2nd Sh. I	10 32	
				2nd Tr. E.	11 0	
				2nd Sh. E.	13 12	
Sat	27			1st Ec. R.	7 2 56	8 9·4
Sun	28	16 23	Occultation of 83 Virginis (6)	3rd Oc. D.	6 58	8 5·4
		17 19	Reappearance of ditto	2nd Ec. R.	7 34 3	
		11	Conjunction of Mars and Mercury 2° 17′ N.	3rd Oc. R.	9 35	
				3rd Ec. D.	11 46 23	
Mon	29					8 1·4
Tues	30	16	Conjunction of Moon and Mercury 5° 26′ N.			7 57·4
		20	Conjunction of Moon and Mars 2° 33′ N.			
			Saturn's Ring: Major axis=43″·64 Minor axis= 9″·91			

THE PLANETS FOR NOVEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	16 0 16	S. 23 26	6″·4	1 15·5
	9th	16 28 52	S. 24 25½	7″·6	1 12·5
	17th	16 28 16	S. 22 59½	9″·2	0 40·5
	25th	15 46 8	S. 18 12½	9″·6	23 23·1
Venus ...	1st	16 25 54	S. 22 40½	11″·7	1 41·0
	9th	17 8 22	S. 24 14	12″·1	1 51·9
	17th	17 51 31	S. 25 2	12″·5	2 3·5
	25th	18 34 50	S. 25 2½	12″·9	2 15·3
Jupiter ...	1st	0 44 14	N. 3 2	45″·5	9 58·0
	9th	0 41 29	N. 2 46½	44″·9	9 23·8
	17th	0 39 25	N. 2 35½	44″·1	8 50·3
	25th	0 38 5	N. 2 29½	42″·9	8 17·5
Saturn ...	1st	1 34 21	N. 6 52½	18″·0	10 48·0
	9th	1 32 11	N. 6 41	17″·9	10 14·3
	17th	1 30 13	N. 6 30½	17″·8	9 41·0
	25th	1 22 33	N. 6 22½	17″·6	9 7·8
Neptune ...	4th	2 44 7	N. 13 58	...	11 45·7
	20th	2 42 22	N. 13 50	...	10 41·1

Mercury is not well situated for observation, setting about forty minutes after the sun at the beginning of the month, the interval decreasing.

Venus sets about an hour and a quarter after the sun at the beginning of the month, the interval increasing.

Jupiter is in a very favourable position for observation, being visible on the 1st, till four hours after midnight, the interval decreasing.

Saturn can be well observed throughout the month, setting about five hours and three-quarters after midnight on the 1st, the interval decreasing.

DUN ECHT CIRCULAR, No. 9.

The Astronomer-Royal has kindly forwarded the following telegram:

A large Comet was discovered by Lewis Swift, at Rochester, New York, on Monday, in—

$$\alpha = 21\text{h.}\ 30\text{m.}$$
$$\delta = +18°.$$

Motion slow, probably north-west. RALPH COPELAND.

Lord Lindsay's Observatory,
Dun Echt: 1880, October 14.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 216. DECEMBER. 1880.

ROYAL ASTRONOMICAL SOCIETY.

Session 1880—81.

First Meeting after the long vacation, November 12, 1880.

Edwin Dunkin, Esq., F.R.S., *Vice-President*, in the Chair.

Secretaries—W. H. M. Christie, Esq., and J. W. L. Glaisher, Esq.

The minutes of the preceding meeting were read and confirmed.

The following gentlemen were duly elected Fellows of the Society:—

The Rev. F. B. Allison, M.A., Grammar School, Chesterfield.

Benj. Francis Cobb, Esq., 79, Cornhill, and 148, Cornwall Road, W.

Henry Augustus Severn, Esq., 9, Earl's Court Road, Kensington, and Devalah, Madras.

Mr. Christie announced that 146 presents had been received. Amongst them is a copy of the works of Laplace, presented by the French Academy; 11 vols. of the Washington Ephemeris, and a photograph of a drawing by the Rev. Cooper Key, of a dark spot on Jupiter, similar to the red spots at present seen.

The Astronomer-Royal was called on to explain two photographs of an instrument of Flamsteed's which he had brought to the Society.

He said: Before referring to the instrument I desire to say that I have been struck with the fact that in recent discussions as to the colours of stars, no appeal has been made to Admiral Smyth's *Sidereal Chromatics*. Among other things the book contains a careful statement of the methods of determining the colours, with a plate of various tints which might be used as standards to which to refer estimates of colour.

My principal object in addressing you at this time is to call attention to two pictures which are now on the walls of the room. About forty years ago there were presented to me by my friend Mr. Francis Baily, two engravings of an instrument of Flamsteed's. There are upon them letters and references, referring evidently to part of an elaborate description. But Mr. Baily told me he had never been able to see any copy of the description, nor have I been able to see any copy, or even to hear of any, and I have reason to believe that the copies of these two pictures, which I hold at the Royal Observatory at Greenwich, are unique. I was anxious that they should not remain in that state, and therefore I have had photographic copies taken of them by the Woodbury Company; and they have been taken with a degree of exactness, even to the minutest mark of the engraver's hatching, that I could not have conceived possible. But while I exhibit these photographs to the Society, I am sorry I cannot leave them in their possession; I can, however, leave them as long as the authorities of the Society think they may be useful. The singularity of that instrument shows the turning point of astronomy, and why it was necessary then, but why it is not necessary now. We all know it is easy enough now to get the north polar distances of objects, and so it always was. There was no difficulty when they had a meridian instrument, and a graduated circle to take the altitude; but the right ascensions have always been a trouble. If you consider astronomy in its very rude state you will see that right ascensions could not be got readily when there were no clocks. From the time I speak down to the time of Halley, there was no clock upon which reliance could be placed. Attention was called by Baily to the fact that Halley in his observations from time to time used a clock wound up by a string, which was probably not any better than the alarums now common.

Mr. Marth directed the attention of the Astronomer-Royal to a volume of Flamsteed's works containing drawings similar to the photographs exhibited.

The Astronomer-Royal: I was aware that in the first volume of Flamsteed's great work there are those small pictures evidently of the same instrument, but they are not the pictures I am referring to. In the absence of clocks upon which reliance could be placed it is not easy to see how the differences of right ascension could be determined. The differences were got in this manner:—Knowing the polar distances of stars and taking the angular distance of one star from another, you can compute the differences of right ascension, and so on all round the celestial globe, so that the differences were got by measuring the angular distances from star to star. Now, the question was, what sort of instrument would we use for such a purpose? It must of course be capable of measuring angles, but how was it constructed? We know after a certain time how it was constructed. There is a valuable work by old Tycho Brahe, his *Astronomiæ Instauratæ Mechanica.* That work contains the engravings of instruments, and one of them is an instrument for this purpose. It is a frame turning upon a vertical axis, and has a quadrant or a sextant whose plane could be turned in different directions, and in that way he had a convenient method of determining the distances between any two stars. But Flamsteed improved upon this instrument, by using, not a vertical axis, but a polar axis, and it is in that state that the instrument is represented in the photographs. And in these, as well as in the engravings, nearly every detail of the construction can be traced so well that a modern instrument-maker could make one like it. These are the general features of the instrument. The polar axis projects beyond its bearing (which would make it unsteady if it were not very strong), and the polar axis has at the top what may be called a T head, provided we can give some latitude to that expression. If it had strictly a T head of the ordinary type to carry the plane of the quadrant or sextant, then the stars which could be reached by the sextant would be but few, and it would not pass through certain stars which it might be desirable to reach. Accordingly the top of this piece is not a T head of the ordinary sort, unless you consider it as an italic *T*, so that the head will admit of being inclined. There is a graduated circle and a screw by which the position of the T head could be inclined to the polar axis, and then the T head has a pivot in working the plane of the sextant. The radius of the sextant is seven feet. You are well aware that the idea of using the complete circle had not entered people's heads, and their instruments were limited in working to arcs of 60 degrees. This sextant, turning upon the pivot, carried two telescopes to be used in the ordinary way. Now, the first volume of Flamsteed's observations contains measures and distances made in that way, and the consequent determination of differences of right ascension. Of course it was desirable not to use this method for every object. It was used for the great stars limited in number, and it cannot be supposed that every little object would be treated in the same way. For the smaller stars they would use the transit instrument and a clock that might go for an hour or so, and then you would arrive at another star whose place was sufficiently well determined. That was the state of astronomy at that time; and, as I mentioned, this is probably the latest instrument that was made on that principle, and, therefore, I thought it worthy the attention of the Society. It was in some of the unhappy circumstances into which Flamsteed's disposition led astronomy at that time, and probably at the time when he speaks of having made a sacrifice for heavenly truth by burning everything, that I suppose the instrument may have been destroyed as well as the descriptions of it, which I have no doubt were printed, for there is no trace of them anywhere amongst the MSS. of Flamsteed. Flamsteed was succeeded by Halley, who relied solely upon places of the principal stars determined by Flamsteed, and filled up the interspaces where necessary with that curious transit instrument of his preserved in the Royal Observatory, which has two axes of unequal length. And things seemed likely to go on in this manner, when there arose a man who reformed astronomy:—a man who has not, I think, received the credit that is due to him,—I mean the clock maker, Graham. Graham introduced clocks which would go, not for a few hours, but for many days, and he also introduced two important principles in the construction of clocks:—One the conpensation or gridiron pendulum, which is equal to any that was ever made, subject only to the fault that sometimes the bars, if not properly made, stick a little, but that is merely a fault in workmanship; which he afterwards improved in the form of the mercurial pendulum. The other important improvement was the dead-beat escapement. The advantages of these two improvements in clock-making were recognised very quickly, and adopted everywhere, and astronomy from that time became a different science,—a science of exclusively meridional observations; for from that time, for large stars as well as small stars, observers used a transit instrument and clock for the determination of the differences of right ascension. I think it is due to the memory of Graham to say he must have carefully studied the action of the maintaining power on the vibrations of the pendulum. No one seems to have had an idea as to the proper going of a clock-pendulum, or that it was necessary that the impulse should be at the centre of the arc of vibration; and no theory, so far as I know, was ever published on the subject, though Graham must have had it in his head, till a paper was published in the Cambridge Philosophical Transactions about forty years ago. Astronomy immediately went on in a different manner, and it assumed at once a form which it has kept up to the present time. Advantage was taken of the fact by Bradley, who worked in a way which excited the admiration of astronomers; but the great reformation was due to Graham, a clock maker of most superior talents and acquirements, and who was not only a clock maker, but a maker of great quadrants and instruments of that sort, such as have never been surpassed. He was the first man to discover the diurnal changes of magnetism, and the irregularities of those changes; he is the only working man who has been buried in Westminster Abbey; and on the occasion of his funeral it was followed by all the members of the Royal Society. I regard this instrument as the last of the ancient class, a verified specimen showing the change which took place under the auspices of Graham, and which has been maintained to the present time. I thought this statement might be interesting to the Society, as calling their attention to one of the most important epochs in meridional astronomy. (Applause.) I shall leave the pictures here as long as anyone wants them for use.

Mr. Knobel: As to the antiquity of Right Ascensions, it may be a matter of small interest to know that the earliest catalogue of Right Ascensions of which we have any account was published before Tycho Brahe's time, for the epoch 1280, Aboul Hassan's Table of the Co-ascendants of stars, when we have a catalogue of 180 stars, the Right Ascensions being measured, not from the Equinox, but from the solstitial colure. They were, probably, determined by an angular instrument, similar to that used by astronomers at a later date. With regard to Admiral Smyth's "Sidereal Chromatics," which has been handed round, I have had occasion to make several references to it lately.*

Captain Noble: With reference to Smyth's "Sidereal Chromatics," it is better known, I think, than Sir George Airy imagines. I have attempted to use it myself, but I have found a difficulty when employing it by the light of a bull's-eye lantern, the colours being different from the same colours as seen in the day time.

Mr. Ranyard: With regard to the Sidereal Chromatics it may be worth mentioning that some time ago I purchased what booksellers call the "remainder" of that work, about 80 or 90 copies, and there are now some 20 copies left, which are in the Society, and are at the service of anybody likely to make use of them who will apply to Mr. Wesley for them. I gave the other copies to double-star observers who I thought would be likely to use them, but I conclude that the value of the book is now much less than it was, for eye-estimates of colour are now superseded by the much more accurate classification of stars by the spectroscope.

The Astronomer-Royal: It is a collection of a great number of observations upon a great many stars by several different persons. It is that which gives it its peculiar value.

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* Vide *Monthly Notices*, June, 1880, p. 538.

Mr. Sadler: I think the collection is mostly derived from Sestini. It is difficult to understand how anyone could compare the colour of a star with this scale of Admiral Smyth's.

Mr. Dunkin: This work of Admiral Smyth's on sidereal chromatics was really distributed in a great degree by the Admiral himself to the principal astronomers. I have a copy myself which was presented at the time of publication. I think it is a work of very great value, and I quite agree with the Astronomer-Royal in drawing attention to it, because it was really valuable for the time at which the observations were made. We must not measure the book by what it is worth now, but what it was worth when published. With regard to the Astronomer-Royal's paper, I think he has made a most interesting statement. As an old assistant of the Astronomer-Royal, who has worked under him for more than forty years, I know pretty well what the instruments now at the Royal Observatory are, but when the Astronomer-Royal showed me those most interesting plates and photographs, I was really astonished at their beauty; every minute detail is given, and every little spot on the original plate is reproduced. The statement of the Astronomer-Royal is most interesting to us, because it shows the history of observations from almost the commencement of the Observatory. He has told us how right ascensions were observed long before clocks were invented, and not only is his statement interesting on that account, but also as showing that those old astronomers, whom we look upon almost as astrologers, knew more than we are sometimes apt to give them credit for, and the amount of originality which they have shown in all their observations and inventions is really astonishing to us now living in the nineteenth century.

The thanks of the Society were voted to the Astronomer-Royal for his paper.

A paper was read by Mr. Neison pointing out an error in one of his late papers, in which, by taking too small a value for the constant of parallax, he had given a value for the mean semi-diameter of the moon some six-tenths of a second of arc too small. He expressed his regret to the Fellows of the Society, and especially to Professor Pritchard, for having temporarily misled them.

Mr. Knott read a paper entitled *Observations of Ceraski's variable star in Cepheus*, illustrated by a diagram showing the changes in magnitude of the star on the evenings of October 23 and November 2. The period of the star appears to be 2½ days, or rather less.

It appeared from the diagram and Mr. Knott's explanation that the star is of the *Algol* type. and remains at its maximum brightness until within a few hours of its minimum, when it rapidly decreases in brightness, remains at its minimum brightness for a few hours, and then rapidly increases in brightness again till it attains its maximum brightness. Mr. Knott mentioned that he had noticed that when the star is near to its minimum brightness it appears to be of a ruddy tint.

In reply to the Astronomer-Royal, Mr. Knott said that the period of 2½ had been determined by other observers. It was at first supposed to vary in a period of five days. He had only observed at present at two successive minima, and so could not speak from his own experience precisely as to the period.

Mr. De La Rue observed that the change of colour noticed might be due to absorption caused by a nebulous mass revolving around the star.

Mr. Knott: Perhaps caused by absorption. It is one of an interesting class of variables that we know little about.

Capt. Noble: I should like to offer a suggestion to Oxford or any other observatory. It is that they should attempt to determine the parallax of these stars of the Algol type that have short periods, because it would be curious to ascertain the scale of these systems. I am referring to what Professor Pickering has written about the variability of these stars as being caused by a dark body revolving about a primary in a very short period. If there is anything in that theory it would be very important to determine the scale of these systems, which can only be done by getting the parallax of these stars.

Mr. Bidder: Has the spectrum been examined of this variable star of Ceraski's by the spectroscope or open prism?

Mr. Knott: By the spectroscope at Dun Echt, and when it was about its minimum the blue of the spectrum died down and the red was intensified. The blue was more conspicuous when it was at maximum.

Mr. Neison showed by a diagram on the black board that the size of the non-luminous object that would be necessary to bring about such an eclipse would be very large compared with the luminous star, and he thought it improbable that there should be so large a mass of non-luminous matter adjacent to a luminous star.

Mr. Ranyard said: It does not follow that because the non-luminous object is large compared with the star that its mass is comparable with that of the star. Professor Pickering has thrown out a very ingenious idea, with regard to the possibility of determining the mass of these non-luminous objects, as compared with the stars with which they are associated. If the density of the non-luminous body is not small compared with the density of the star, the common centre of gravity of the two bodies will be outside the bright star, and it will consequently describe an orbit of appreciable magnitude. Professor Pickering suggests that the spectroscope might be made use of to determine the approach and recession of the bright star when in opposite parts of its orbit, and from the velocity in miles thus determined the dimensions of the orbit of the bright star could be calculated. If no shift could be detected we should know that the velocity of the bright star in its orbit was small, and we might assume either that the density of the dark body is small compared with that of the bright star, or that the scale of the whole system is small, in which case a large parallax of the bright star should be detectable.

Mr. Brett: If there were such a displacement of the star it would subtend so small an angle that no one could discover it.

Thanks were voted to Mr. Knott for his paper.

Mr. Common was called upon to read a paper describing the mode he had adopted of mounting his 3-foot reflector. He said: I have here some large diagrams that I have prepared.

Mr. Brett: Draw it on the black board.

Mr. Common: I will do anything to impress your mind with a proper idea of the mounting, and I shall endeavour to make it a little more clear than I did last time. After selecting the Newtonian principle, I laid certain conditions down for the better performance of the reflector. There was to be no tube, no mass of little pulleys; an equatorial motion, safe, steady, and easily adjusted platform, allowing two hours observation with no motion except that given from the observer's place, &c. An endeavour has been made in the mounting to satisfy these conditions, and the result is such a departure from the old style that I am induced to think an account of it may be useful, and that the plan is suitable not only to this, but to telescopes of a larger kind.

Mr. Common proceeded to describe his plan with the aid of his diagrams, spending some time in an explanation in regard to the mounting of the mirror. The failure of the Paris reflector might be attributed entirely to the way the mirror is mounted. It rests on a wooden rim all round, causing a great strain on the central portions of the mirror, and accounting for its bad performance. This fault was avoided in the mirror under discussion by means of a system of levers pressing on the back of the mirror, so that every part of it was supported. The edge of the mirror was supported by metal bands, so that in all positions the strain was about equal. It was necessary to have a cover for the mirror to exclude damp air.

Mr. Common also read two other papers, one entitled *Note on* *the Nebulæ near Merope*, and the other *Note on a photograph of Jupiter*. He mentioned that he had succeeded in obtaining a photograph of Jupiter showing a shadow of one of the satellites on its disc.

Mr. Brett: The subject of reflecting telescopes is now before us, and all those who have used them know that there is one great objection to them, owing to a deposition of dew on the small mirror. Now the dew must be either falling upon the little mirror or rising to it; or else it must be floating as warm vapour, from which it condenses the moisture upon itself. If the dew is falling it cannot fall upon the mirror, because it faces the earth, and if the dew is rising it cannot come upon the little mirror because it is enclosed in a tube, at the bottom of which is the great mirror, and if the moisture is floating in the air and surrounds the little mirror, there is no reason that it should deposit itself, because the temperature of the little mirror must be the same as the medium in which it stands, therefore the deposit on the mirror is not accounted for by the usual explanation. The explanation, as it appears to me, is simple; namely, that the observer acts like a cauldron of boiling water, and produces a great deal of steam, and the little mirror is naturally cooler than the observer, and a great deal of the steam must be deposited upon it, and the only cure I have found practicable is to look to windward. If the observer stands with his face towards the wind, this cloud of moisture which emanates from himself will be blown away, and will not be deposited on the mirror, and he will observe with comfort. The greatest of all reflecting telescope observers, Mr. Lascelles, would carry a collection of small pieces of metal in his pocket, and would slip them in behind the little mirror, so as to raise the temperature of the little mirror to that of himself, and in practice it answered very well, but there is no such contrivance in my own telescope, so I have arranged the place of the eyepiece on the leeward side of the telescope, and that answers the purpose.

Mr. Common: One minute, Mr. President. There is a great deal of truth in what Mr. Brett says. I mentioned in the paper that it would be better to have a screen for the observer, that would run along the platform so as not only to protect the observer from the inclemency of the weather, but also to serve as a protection for the telescope from the observer and his lamp. There is no doubt that heat from the body has some effect. Mr. Brett's experience as to whether observers steam at their work is rather different to mine. (Laughter.) Generally in observing I find it anything but steaming work. With regard to floating moisture the best thing I find is a dew-cap for the flat. I have tried placing a wire behind the flat, and making it white hot with an electric current, so as to raise the temperature of the flat, but the most efficient protection against the deposit of dew and the steam of the observer, or whatever it may be, that I know of, is a dew-cap for the flat.

Mr. Dunkin: I am sure anyone looking at Mr. Common's drawings will see that he has taken a great deal of pains to get a good mounting for his telescope. With regard to the steaming process, I may be allowed to say that I have frequently steamed in making astronomical observations, and I should advise Mr. Common to go to Greenwich some close night, and let him observe with the altazimuth, and try to get his observations quickly, and by the time he has done he will steam pretty well.

Captain Noble read a paper entitled *A Phenomenon of Jupiter's Satellites.* On the night of October the 18th, in observing Jupiter he noticed two dark spots, one of which was decidedly darker than the other. He at first supposed the darker spot to be the shadow of one of the satellites, and that the other spot was satellite No. III. in transit; but on referring to the *Nautical Almanac* he found that they were the shadows of satellites I. and II. He was much surprised at this, as there was such a marked difference between the darkness of the two spots that he thought that it could not be a mere effect of contrast. The black spot was near to the southern edge of the equatorial belt, while the other which was chocolate coloured was further south. The only method of accounting for this difference of colour which occurred to him, was that one part of the planet must be shining with its own intrinsic light, which rendered the area where the sun's light was cut off visible as a reddish brown spot.

Mr. Campbell said he had observed the shadows of the two satellites. He had seen exactly the same phenomena as Captain Noble had described. He believed a similar appearance had been described in *Webb's Celestial objects for common telescopes.*

Mr. Christie read a paper by Mr. Baxendell, on a new nebula of considerable area in Aquarius, which he had discovered.

Capt. Noble asked what aperture Mr. Baxendell had used.

Mr. Ranyard: The aperture would not matter much in observing a nebula of large area, for you cannot increase the brightness of an object of sensible magnitude by means of a telescope. Light is always lost even when the whole pencil of light coming from the object-glass is used. The larger the telescope the higher must be the power employed to make use of the whole pencil, and, therefore, for such nebulæ clearness of sky is of the greatest importance, so that they would have a great advantage over us in Italy and America.

Mr. Christie read a paper by Messrs. Burton and Grubb, *On a new form of Ghost micrometer*. The paper stated that the instrument somewhat resembled the micrometer described by Mr. Bidder in 1874.

Mr. Bidder said the principle of the micrometer just explained was essentially the same as his own; he was naturally, as a father, proud of his own bantling, and he did not think that they had made any improvement on it. One peculiarity in his instrument, which was referred to as a disadvantage, he considered as one of the greatest luxuries, namely, the power of moving the rectangular prism so that the rays were turned in any required direction. He thought his own form offered the most advantages.

Mr. Downing read a paper *On the value of the diameter of Mars*. The diameter assumed in the *Nautical Almanac*, viz., 11″·1, appears to be considerably too large, it is desirable, therefore, that the value found from the observations made with the Greenwich transit-circle should be investigated, especially for use in cases where only one limb of the planet is observed. From 1851 to 1865 the value of the diameter assumed in the *Nautical Almanac* was 8″·87; and from the observations of vertical diameter made during those years, the true diameter is 9″.625. Since the year 1866 the value assumed is 11″·1, and the true value is 9″·761. Combining these two according to their weights, the diameter derived from all the observations of vertical diameter made with the transit circle is 9″·697 ± 0″·107.

Mr. Stone read a paper *On the determination of the co-efficient of the parallactic inequality in the expression for the moon's longitude*. It consists of two parts, one an examination of the corrections which Mr. Neison had applied to Mr. Stone's result, arising from a misconception of the method he had employed, and the other a discussion of the results obtained by Messrs. Neison and Campbell.

Mr. Neison, in reply, said he had dealt with the abstract given of Mr. Stone's paper. That abstract said that Mr. Stone did one thing, but did not say any more, and he therefore assumed that Mr. Stone had not done anything more. But now it seemed that Mr. Stone had done something else, and therefore the correction he had applied was not satisfactory.

The Chairman said the paper of Mr. Stone was very important, and when printed they would be able to have Mr. Neison's answer. What they wanted was to get at the truth.

Mr. Christie announced the receipt of a telegram from Washington, with regard to the last comet discovered by Dr. Lohse, at Lord Lindsay's observatory at Dun Echt, and stating that it was the same as that discovered by Mr. Lewis Swift on Oct. the 10th, of which nothing had been heard since. Mr. Swift had found that this comet probably corresponded with Comet III. 1869, and it was therefore a comet of eleven years period.

The following papers were also announced and partly read:

T. H. Safford: *Elements of Comet Schäberle* (1880 II.).

H. C. Russell: *Southern double stars.*

C. R. Powalky: *Comparison of the observations of the sun made at Washington,* 1866—75, *with Hansen's Tables.*

G. M. Seabroke: *Third catalogue of micrometrical measures of double stars made at the Temple Observatory, Rugby.*

S. W. Burnham: *The discovery of κ Pegasi as a close double star.*

Admiral Sir E. Ommaney: *The August Perseids.*

J. W. L. Glaisher: *On the method of least squares.*

E. Sang: *On the possible performance of an object-glass for star-gazing.*

D. Gill: *Observations of Comet I.,* 1880, *made at the Royal Observatory, Cape of Good Hope.*

E. F. Sawyer: *The July Aquariads,* 1880.

C. F. Varley: *Observations of a meteor Nov. 4th,* 1880.

R. S. Ball: *On the annual parallax of the star* P. III. 242.

W. H. M. Christie: *On the spectrum of Comet* 1880 *d.* (*Hartwig*)

W. F. Denning: *Saturn in October,* 1880.

W. F. Denning: *The motions and varieties of the Jovian spots.*

J. W. L. Glaisher: *Addition to a paper on the method of least squares.*

C. Pritchard: *Cometary observations made at the University Observatory, Oxford.*

A. V. Nursinga Row: *Observations of the red spot on Jupiter.*

L. S. Little: *Telegraphic determination of the longitude of Shanghai.*

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

ENDOWMENT OF RESEARCH,

The article by Prof. Piazzi Smyth, published in your last number, frankly states the failures which have hitherto attended his attempts to predict hot and cold periods, from observations of sun-spots and rock thermometers, but he nevertheless thinks that the

Government should cause "observations of sun-spots to be multiplied," and should "engage mathematical theorists" to pursue a line of research which has hitherto proved so unproductive. And *he* appears to believe that the Government would have already adopted his suggestion, if it were not "for a small knot of writers" "who have taken to abuse sun-spot observations, and to warning Government and the people, too, against sun-spot observers."

Prof. Piazzi Smyth cannot, I think, have sufficiently considered the arguments which the promoter of such a speculative research ought to be prepared to urge if he wishes to induce a conscientious minister to tax the non-astronomical part of the community in order that an astronomical theory may be tested.

In the first place it should be shown that the theory is generally accepted by those who have given their attention to the subject, but at present Prof. Smyth is very far from having obtained such a consensus of opinion. He owns that he has not himself been able to make one successful prediction, and, as far as my memory serves me, none of the other writers on this modern development of astrology have been as bold as Prof. Smyth in making predictions, but have contented themselves with the much safer task of discussing past records; there are, therefore, at present no accomplished predictions to point to, and there is not even a consensus of opinion with respect to past records, for some of his competitors in the field believe in an increase of rainfall and storms about periods of sun-spot maxima, while others are equally confident that the same phenomena are associated with minimum sun-spot periods, and a fact that is of still greater importance must be taken into account, namely, that many conscientious meteorologists have examined their records, and have come to the conclusion that there is no connection between solar cycles and terrestrial temperatures, rainfall, and storms.

But let us suppose that Prof. Smyth had succeeded in showing that most astronomers and meteorologists agree with him as to his theory, it would then be necessary to show that the predictions when made would be useful to the public, and that none but Government paid officials could or would make such predictions successfully. First, as to the use of such predictions. We are very far from being able to predict when periods of sun-spot maxima and minima will occur. According to Dr. Wolf's valuable table published in Vol. XLIII. of the Memoirs of the Royal Astronomical Society there were 16·1 years between the successive maxima of 1788·1 and 1804·2, while a period of only 7·3 years elapsed between the well-determined maxima of 1829·9 and 1832·2, and it will be evident that the terrestrial periods could not by means of the theory be predicted with greater certainty than the solar periods. Probably nothing is more certainly known about sun-spots than the fact of the irregularity of the intervals between the periods of their maximum and minimum development, and the complicated law which governs that irregularity is at present as unknown as the laws which govern the recurrence of storms and other terrestrial phenomena.

But let us assume that this difficulty could be successfully surmounted, and that sun spot maximum and minimum periods could be predicted with the certainty which may be attained in predicting a solar eclipse. It would then be doubtful whether such predictions would be of use to farmers and ship owners, or the community at large. For example, what advantage would it be to a ship owner to know that in a particular year his ship would probably encounter seven storms during a certain voyage, while in another year during a similar voyage it would probably only encounter three storms? Such information would be very inferior in value to the deductions which may be drawn from the observation of a falling barometer, or from an observation as to the direction of the wind on the outskirts of a cyclonic storm. A farmer needs to know whether he will be able to carry his corn or his hay on the morrow, and general predictions with regard to the whole season would not affect him, unless they affected the price of corn from year to year. But long ago the elder Herschel, and more recently Carrington* with greater detail, showed that there was no connection between the price of corn and sun spots.

But in order to meet, as far as possible, Prof. Smyth, Mr. Lockyer, and the other supporters of this strange theory, I will assume for a moment that the poor of Whitechapel (who would have to contribute their quota towards the support of the proposed new Government department) would be benefited by such predictions. It would still be necessary for the promoters of the endowment of such an observatory at the expense of the nation to show that existing observatories, or private individuals, could not be found to make the necessary observations. I am not one of those who believe that there is any magic about a Government official, which will enable him to see any more, or arrive at any better conclusions, than an ordinary man; but it would seem that Prof. Smyth thinks that there is something of the kind, for he writes on page 269 "I have been probing the authorities at Greenwich deeply, I fear that they may think rather troublesomely." . . . . "but I applied to them because they are observing the sun spots officially." And there are many other people who imagine that there is some mystic power about a duly appointed Government official, but a moment's consideration will show that scientific research is rather more likely to be carried on with energy and enthusiasm by private workers than by public officials. The individual responsibility enjoyed by private workers is a valuable stimulus to accuracy, so that from a purely scientific point of view it would probably be better that researches should be carried on by private individuals rather than by officials working under the heads of their departments, or even working separately under the Government control, which must necessarily accompany Government help.

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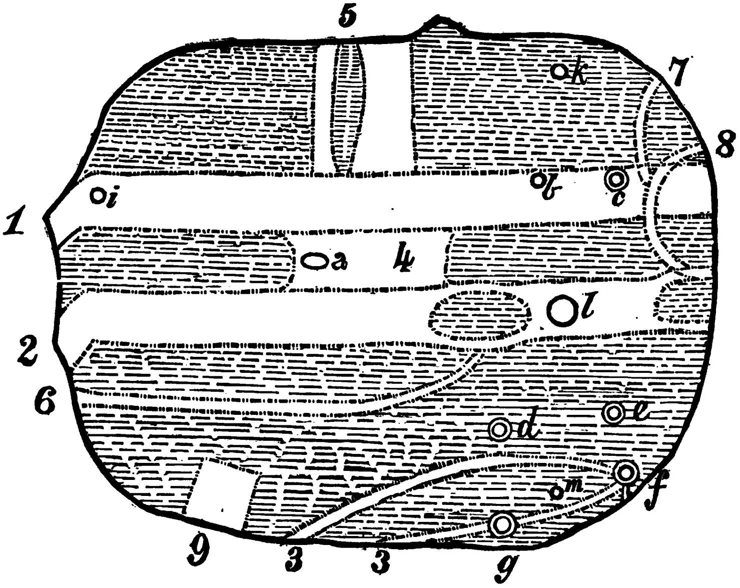
*See "Carrington's observations of solar spots." 4to. 1863. pp. 247, 248.

A new scientific department once created can only with great difficulty be abolished, and there is a danger of officials finding a *raison d'être* for their existence that is not likely to be favourable to the scientific spirit which it is important to foster. The bulky volumes issued by official departments form a growing tax upon the time of the scientific worker, who endeavours before he writes to discover what has already been accomplished in the same field. He dare not omit to examine these mountains of paper lest something of importance should be lost, but every unnecessary page inflicts a tax upon his time, which is probably more hurtful to the community than the money wasted in printing the unnecessary matter.

The patronage connected with Government appointments must be distributed either by Parliament or by some Government official, possibly with the advice of those who are considered scientific experts, but this would not be without its attendant disadvantages upon the body of voluntary workers as well as upon the scientific societies. Many of the unselected voluntary workers would believe that if they had been more men of the world and less men of science, they might have succeeded better, and such a belief will have a tendency to shape their conduct. The moral atmosphere of the learned societies would also be injuriously affected. These societies would tend to become machines for distributing patronage, fields where string-pulling and diplomacy would occupy the time and the thoughts of the individuals brought together, rather than the pursuit of the sciences which they were founded to promote. At present one cannot see without regret how the time of some most valuable workers is wasted in the distribution of Government grants and patronage, while they are exposed to the attempts of charlatans and self-seekers to obtain personal influence over them, which cannot be beneficial to the distributors of the patronage or to the scientific communities in which the patronage is vested. Mr. Anthony Trollope in his life of Thackeray says, "It may be doubted whether a special rank of its own would be good for literature such as that which is achieved by the happy possessors of the forty chairs of the Academy in France. Even though they had an angel to make the choice—which they have not—that angel would do more harm to the excluded than good to the selected."

A. C. RANYARD.

KEY PLAN OF ARCHIMEDES.



◎ Spots seen as crater-cones.

○ White spots.

THE CRATER ARCHIMEDES.

The accompanying Key Plan is based upon seventy-two observations of this crater, made between November, 1879, and October, 1880, by the Rev. F. B. Allison, of Chesterfield, and myself. On it are shown eleven light streaks, and eleven spots; six of which have been seen as crater-cones.

In observing light streaks upon the floors of craters it is very important that there should be previous drawings, in order that, an interval of several years having elapsed between the two sets, they may be compared together for the purpose of detecting any change in the arrangement of the markings. In the case of Archimedes we are singularly fortunate in this respect, some excellent drawings having been made during the years 1860, and 1861, by Mr. Knott. During the twenty years, 1860 to 1880, although little, if any, change is noticed in the craterlets, there is a decided alteration in the light markings; it is evident that the N. portion of the floor does *not* represent the same appearance that it did in 1860. It seems to have become darker. I have copies of three drawings, by Mr. Knott, made in 1860, on March 2d. 8h., moon's age 10d.; March 5d. 10h. to 10h. 30m., M. A., 13·1d. and July 6d. 11h. 40m. to 12h. 30m., M. A., 17·8d. On these drawings the S. and central bright zones (marked 1 and 2 on Key Map), are clearly shown; also streak 6, though in a somewhat different form. But 5 is not given; yet not only have we seen it frequently during the past twelve months, but we have found it considerably easier than some of the minute spots which Mr. Knott has drawn; if not a new streak, its visibility must have greatly increased. The two dark patches on the central bright zone (2) were seen in 1860. So far the changes are slight and unimportant; not so, however, when we examine the N. portion of the floor. Mr. Knott saw the N. bright zone as a broad streak extending across the whole floor from E. to W., and occupying the greater part of the N. portion; quite equal in breadth to the S. or central bright zones. Turning to the Key Map we find in the place of the N. bright zone merely the two faint, narrow streaks 3 and 3 in the N.E. corner, and of these two the N. is probably a low mountain ridge. Streak 9 is hardly ever visible, (it is considered to be raised ground by Rev. F. B. Allison), and streak 6 is so close to the central bright zone, that it can hardly be separated from it; thus in the N. there is between ½ and ⅓ of the entire floor which on the average presents a dark appearance. The question arises, whether this fading away of the N. bright zone has been sudden or gradual; the following extract from Mr. Neison's *Moon*, under the head of Archimedes, seems to indicate a period when this streak was passing from its bright appearance in 1860 to the narrow and faint marking which is its present aspect. "It is traversed by unequally bright streaks diverting it into seven zones, though the northern clear zone is particularly difficult to see."

The streaks.—Starting from the S. wall we first come to the double streak, 5; during the thirty observations which have been made of it, on only a few occasions has its double appearance been noticed. The streak was discovered by Rev. F. B. Allison, on December 25th, 1879. Next comes the S. bright zone. No. 1, nearly always visible and showing but little variation, except that its W. end seems generally to bend sharply northwards, and its E. end has been occupied by the curved streaks 7 and 8 on the three occasions when these difficult objects have been visible; they were first seen by me on March 28d. 1880. Rev. F. B. Allison observed them on July 24d., and I saw them again on October 20d. Streak 4, connecting 1 and 2, has seldom been missed by us, although not shown by Mr. Knott. The central bright zone, streak 2, is, perhaps, the most interesting feature on the floor; like the S. zone, its W. end is sharply bent; upon it, N.E. of the connecting streak 4, is a curious marking, distinct, dark, and oval-shaped; out of sixty-three observations of the zone this dark patch has been seen thirty-seven times. At the E. end of the zone is another marking, similar to the central one, but fainter, having been observed only nineteen times. Immediately N. of the central zone is the narrower streak 6; eighteen observations have been made of it, but the whole has seldom been seen at once; it is a faint object and difficult to trace, especially the E. half. Streak 9, a broad and very short streak, seen on five occasions by the Rev. F. B. Allison. Streaks 3, the remnant of the once conspicuous N. bright zone. The S. streak 3 is considerably fainter than streaks 1 or 2; thus on December 22nd, 8h, 1879, it was "not visible, although the S. and central bright zones were seen" by Rev. F. B. Allison; it has been observed on fifty-two occasions. The N. streak 3 is more difficult, having been seen only twenty-one times. The following interesting observation throws some light on the probable nature of these streaks. 1880, October 11d. 7h. The Rev. F. B. Allison, while observing Archimedes soon after sun-rise, saw a roughness on the floor about the neighbourhood of the N. and S. sides of the N. bright zone, which were *not* seen.

The craterlets and white spots.—Ninety-two observations have been made with the following results:—*a.* Seven observations. A small, oval, white patch. *b.* On October 14d. 10h. 30m. Seen by me as a minute white spot. *c.* *d.* and *e.* Fourteen observations; they usually appeared as minute, white spots. *f.* Thirty-five observations. The peculiar appearances of this craterlet were discussed in the June number of the *Register*. *g.* Eleven observations. A white spot varying in size and distinctness of outline. *i.* and *k.* were seen by me on October 14d. 10h. 30m. *l.* Sometimes hazy and ill-defined; sometimes brilliant and distinct. Its remarkable variability will be best seen from this table. In April, 1880, its visibility = ·5; in May = ·4. In June it rose to 1·0; in July = 1·0; in September = 1·0. In October it fell to ·3. During the maximum of brilliancy it was quite conspicuous. It was not seen in 1860. The brightness of this spot regularly increases as the sun rises upon Archimedes, reaching its greatest brilliancy about 13d. of the moon's age; it then fades. Another spot, *m*, not shown by Mr. Knott, was seen by me on October 14d. 10h. 30m. Perhaps the most difficult object on the floor and, probably, a very minute crater-cone.

The order of visibility deduced from the above observations is—

f = ·486 *l* = ·327 *g* = ·152 *d* = ·125.
a = ·083 *e* = ·041 *c* = ·027 *b* = ·013.
i = ·013 *k* = ·013 *m* = ·013

In conclusion, may I ask those gentlemen, who are interested in the study of light streaks, to observe, if only occasionally, this very interesting formation. A new series of observations has been commenced with a larger number of observers and instruments, by which means we hope to obtain fresh knowledge of the markings on Archimedes.

The Lodge, Clapham Road, T. P. GRAY.
Bedford:
November 19, 1880.

THE VARIABLE (?) STARS E, F, IN THE TRAPEZIUM OF ORION.

Some account of these stars will be found in the *Astronomical Register* for March and April, 1880. As the wonderful nebula is now becoming visible in the east, the following extract from O. Σ.'s great work[*] on this object is given, in the hope that it may stimulate those observers who have suitable telescopes, leisure, and good skies, to make a careful series of observations during the present apparition.

O. Σ. says that in order to establish the variability of E and F (the 5th and 6th stars), "It suffices to compare the observations made in the spring and autumn of 1857. In the first period F was seen a whole magnitude smaller, in the second a whole magnitude greater than E; and similar observations have been recorded on other different occasions. There can be no doubt, therefore, that one of the two is subject to considerable changes of brightness, but it is still uncertain which. Probably it is F, but unless I am deceived E also is slightly variable." It ought to be added that these results were obtained during a long and careful study of the great nebula and the small stars in and near it in the years 1856—62 at Pulkova with the great refractor. The magnitudes of the stars were compared, and the order of brightness noted on each occasion. The following extracts will indicate the care bestowed on the observations:

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[*] Obs. de la grande Nébuleuse d'Orion.

"1857, Mars 20, 7h. 48m. F est de toute une grandeur plus faible que E."

"1857, Mars 23, 8h. 20m. La sixième étoile du trapèze F est aujourd'hui de très près égale en éclat à E."

"1857, Oct. 27, 2h. 30m.—5h. 0m. F est sans doute plus luisante que E."

"1858, Oct. 28, 5h. 30m. F est considérablement plus faible que E."

"1861, Mars 9, 6h.—8h. E est aujourd'hui considérablement plus luisante que F."

"1861, Mars 15, 6h. 40m.—8h. Bonnes images. F de toute une marche de notre échelle des grandeurs plus faible que E."

With regard to the faint stars, said to have been seen within the trapezium by De Vico and his assistant, it is worth while noting that the instrument used by these observers was a 6¼-in. refractor, and that neither of them could see the sixth star.

Mr. Edward Crossley's Observatory, J. GLEDHILL.
Bermerside, Halifax.

THE GEMINIDS.

Sir,—As the time for the reappearance of these meteors is approaching, I wish to point out a feature of last year's shower which I have not seen mentioned, namely, the definiteness of the shower of Dec. 12, which appeared to me as though it was a distinct shower from that of the 10th and 11th. The centre of the radiant area on the 12th was, at R.A. 112°, N.D. 34°, several degrees from what is considered the average radiant point of the Geminids, and the area was a much smaller one than that of the 10th. The average hourly number of meteors of all kinds on the 12th, between 15h. 20m. and 18h. 43m., was 22, and there was a large proportion of brilliant ones. I have forwarded further particulars to the committee of the British Association, but they have not yet been published.

This year being leap year it is possible the shower may be repeated on the 11th, but in 1876, on Dec. 11, the radiant was very diffuse, according to my observations, while on the 12th there were indications of the more definite radiant of last year. Also in 1877, of the five Geminids I saw on the 12th the paths of four very closely agree with that radiant. B.

HARTWIG'S COMET.

Sir,—I was surprised to see Mr. T. W. Backhouse's letter in your last number.

I received the notice from the Astronomical Society of the discovery of this comet on the morning of the 2nd October, and on looking at the sky at 6h. 45m., G.M.T., of the same evening, immediately found it with a field glass—it soon after became visible to the naked eye.

A rough position with a transit-eyepiece on a 9-inch silver speculum equatorial at 8h., G.M.T., is—

R.A ... 15h. 7m.
Dec. ... $+28^\circ\ 15'.5$

This position differs considerably from that assigned to it by the circular, but as the figures in the telegram were evidently wrong, it was difficult to estimate its probable place.

Its brightness was equal to that of a star of 4th mag., and it had a tail of 2° in length.

On Oct. 3rd it was distinctly fainter, so that it must have been very bright at the time of its discovery on Sept. 29th.

Oct. 9. Much fainter, and tail very small.

Oct. 21. Obtained a glimpse of it in strong moonlight through the 8-inch equatorial of the Liverpool Observatory at Bidston.

Oct. 23. Had a very good observation, and was surprised to see that it still had a tail of about $\frac{3}{4}^\circ$ in length.

Higher Bebington Hall, Yours obediently,
Birkenhead: Nov. 17, 1880. R. C. JOHNSON.

PHOTOGRAPHS OF THE NEBULA IN ORION.

Professor Henry Draper, M.D., writes that "During the night of September 30, 1880, I succeeded in photographing the bright part of the nebula in Orion in the vicinity of the trapezium. The photographs show the mottled appearance of this region distinctly. They were taken by the aid of a triple objective of 11-in. aperture, made by Alvan Clark and Sons, and corrected especially for the photographic rays. The equatorial stand and driving-clock I constructed myself. The exposure was for fifty minutes. I intend at an early date to publish a detailed description of the negatives."

New York: Oct. 2, 1880.

AURORA BOREALIS.

There was a fine display of aurora here on Thursday night, at 10·15 p.m. The northern part of the heavens, from the horizon to near Polaris, was a blaze of fine white light, reaching from time to time from Capella on the east to Cor Caroli westwards. The shooting up of the streamers from a thin line till they became like giant fountains of light was very beautiful. The display gradually died out about 11·30 p.m.

Ilfracombe: August 12, 1880. F.R.A.S.

REMARKS ON COMETS.

In connection with the assumed identity of the southern comet of 1880 with that of 1843, it does not appear to have been noticed that some computers, years ago, assigned a very short period to the 1843 comet. Humboldt (Cosmos, Vol. III., Part II.) in a note on the subject of comets remarks, "Other combinations of Pierce and Clausen even give periods of revolution of 21⅘ or 7⅕ years, sufficient proof of how hazardous it is to trace back the comet of 1843 to the time of the Archon Asteus." It is much to be regretted that our country was not favoured in 1880 with even the partial view that was obtained of the comet of 1843. While the last-named comet appeared with such a splendid nucleus and vast train to the West India Isles, to the Cape, Bombay, Madras, &c., it was partially seen in England, the tail appearing to cut the horizon so soon as the sky in the evening was sufficiently dark to render it visible. On March 17, at 8h., at Epping, in Essex, the train was noticed to reach from the horizon about 5° south of the west, to within a few degrees of Sirius, a distance of about 55° !

Exhaustive as is the excellent catalogue of comets to be found in Chambers' "Descriptive Astronomy," it is possible to add to it. The following will not be found in that work:

B.C. 1150±. A comet recorded in the Assyrian inscriptions, and mentioned by Rev. A. H. Sayce in his "Babylonian Literature." At the time Nebuchadnezzar I. overran Elam, about B.C. 1140, "a comet arose, whose body was bright like the day, while from its luminous body a tail extended, like the sting of a scorpion." It moved from north to south, and was accounted a presage of good fortune. How different from the inference usually drawn from these objects in subsequent ages.

A.D. 706. In third year of king Ethelhard, of Wessex (he seems to have come to the throne in 703), "apparuerunt duæ cometæ . . . una vespere, alia mane; una occidente, alia oriente, portabant autem faciem ignis contra Aquilonis partem . . . apparebantque mense Januario fere per quindenam." Bartholemæi de Cotton, Historia Angl.

916. The same authority informs us of a comet in the time of Edward, son of Alfred, "anno ejus xv apparuit cometa."

1227. "Apparuit populo crux cum crufixcio in aere." Matthew Paris (Abbrev. Chronic.). This is the date given; it can hardly refer to the Chinese observation of 1226.

1618. In an old pamphlet entitled, "A Dissertation on Comets," I find a statement that eight or nine were seen in the single year 1618. Surely this must be an exaggeration.

Abbenhall Rectory, S. J. JOHNSON.
Gloucester: August 2, 1880.

THE PLANETS FOR DECEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	15 30 32	S. 16 17	8″·4	22 43·5
	9th	15 44 28	S. 17 20½	6″·8	22 26·4
	17th	16 21 27	S. 20 2	5″·9	22 31·8
	25th	17 8 5	S. 22 30	5″·3	22 46·8
Venus ...	1st	19 7 6	S. 24 31	13″·2	2 23·8
	9th	19 49 20	S. 23 9	13″·8	2 34·5
	17th	20 30 14	S. 21 5	14″·3	2 43·8
	25th	21 9 34	S. 18 23½	15″·1	2 51·6
Jupiter ...	1st	0 37 37	N. 2 28½	42″·1	7 53·4
	9th	0 37 43	N. 2 31½	41″·1	7 22·1
	17th	0 38 36	N. 2 40	39″·9	6 51·5
	25th	0 40 17	N. 2 53	38″·9	6 21·7
Saturn ...	1st	1 27 31	N. 6 17½	17″·4	8 43·2
	9th	1 26 28	N. 6 13½	17″·2	8 10·7
	17th	1 25 50	N. 6 12	17″·0	7 38·6
	25th	1 25 38	N. 6 13½	16″·8	7 7·0
Neptune ...	2nd	2 41 10	N. 13 45	...	9 52·8
	18th	2 39 51	N. 13 39½	...	8 48·5

Mercury may be observed an hour and three-quarters before sunrise at the beginning of the month, the interval increasing till the middle of the month, after which the interval gradually decreases to one hour before sunset on the 31st.

Venus sets about two hours after the sun on the 1st, the interval increasing.

Jupiter sets about two hours after midnight at the beginning of the month, the interval decreasing.

Saturn sets about three hours and twenty minutes after midnight, the interval decreasing.

ASTRONOMICAL OCCURRENCES FOR DECEMBER, 1880.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Saturn.
Wed	1	14 56	● New Moon Eclipse of the Sun invisible at Greenwich	**1st Tr. I.**	13 49	8 43·2
Thur	2		Sidereal Time at Mean Noon 16h. 46m. 49·13s.	**1st Oc. D.**	11 6	8 39·1
Fri	3		Sun's Meridian Passage 9m. 47·22s. before Mean Noon	**1st Tr. I.** **1st Sh. I.** **1st Tr. E.** **2nd Tr. I.** **1st Sh. E.**	8 17 9 29 10 31 10 47 11 43	8 35·1
Sat	4	6 21	Conjunction of Moon and Venus 4° 3′ S. Uranus at quadrature with the Sun	**1st Oc. D.** **1st Ec. R.**	5 34 8 58 49	8 31·0
Sun	5			**1st Tr. E.** **1st Oc. D.** **1st Sh. E.** **2nd Ec. R.** **3rd Oc. D.**	4 59 5 7 6 0 2 10 10 2 10 39	8 26·9
Mon	6					8 22·9
Tues	7			**2nd Sh. E.**	5 11	8 18·8
Wed	8	6 38	☽ Moon's First Quarter			8 14·8
Thur	9			**3rd Sh. I.** **3rd Sh. E.**	5 53 8 18	8 10·7
Fri	10	5	Conjunction of Moon and Jupiter 7° 6′ S.	**1st Tr. I.** **1st Sh. I.** **1st Tr. E.**	10 8 11 25 10 23	8 6·7
Sat	11	6 9 7 55 5	Near approach of 101 Piscium (6) Near approach of 104 Piscium (6½) Conjunction of Moon and Saturn 7° 53′ S.	**1st Oc. D.** **1st Ec. R.**	7 26 10 54 43	8 2·7
Sun	12	10 39 11 40	**Occultation of B.A.C. 782 (6½)** **Reappearance of ditto**	**1st Sh. I.** **1st Tr. E.** **2nd Oc. D.** **1st Sh. E.**	5 53 6 52 7 36 8 7	7 58·6
Mon	13	5 0 7 54 9 13	**Near approach of ζ Arietis (4½)** **Occultation of τ^2 Arietis (5)** **Reappearance of ditto**	**1st Ec. R.**	5 23 37	7 54·6
Tues	14	3 50 15 47 16 25 16 4 17 3	**Near approach of A Tauri (4½)** **Occultation of υ^1 Tauri (4½)** **Reappearance of ditto** **Occultation of υ^2 Tauri (6)** **Reappearance of ditto** **Illuminated portion of disc of Venus=0″·773** **Illuminated portion of disc of Mars=0·993**	**2nd Sh. I.** **2nd Tr. E.** **2nd Sh. E.**	5 9 5 17 7 50	7 50·6

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Saturn.
Wed	15					7 46·6
Thur	16	13 36 17 51 18 25	○ Full Moon Eclipse of the Moon Occultation of η Geminorum (3½) Reappearance of ditto	3rd Tr. E. 3rd Sh. I.	7 11 9 55	7 42·6
Fri	17	16 57 18 0	Occultation of ζ Geminorum (4) Reappearance of ditto	1st Tr. I.	12 1	7 38·6
Sat	18	11 59 13 13	Occultation of g Geminorum (5½) Reappearance of ditto	1st Oc. D.	9 19	7 34·0
Sun	19		Sidereal Time at Mean Noon 17h. 53m. 50·61s.	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E. 1st Oc. D.	6 29 7 49 8 44 10 3 10 7	7 30·7
Mon	20		Saturn's Ring: Major axis=42″·28 Minor axis= 9″·51	1st Ec. R.	7 19 32	7 26·7
Tues	21		Sun's Meridian Passage 1m. 24·88s. before Mean Noon	2nd Tr. I. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	5 9 7 48 7 51 10 28	7 22·7
Wed	22	13 5 14 10 15 14 16 28	Occultation of B.A.C. 3726 (6) Reappearance of ditto Occultation of 55 Leonis (6) Reappearance of ditto			7 18·8
Thur	23	1	Conjunction of Mars and Mercury 1° 5′ N.	3rd Tr. I. 3rd Tr. E.	8 23 11 6	7 14·8
Fri	24	6 56	☾ Moon's Last Quarter			7 10·9
Sat	25			1st Oc. D.	11 14	7 7·0
Sun	26	17 51	Near approach of B.A.C. 4739 (6½)	1st Tr. I. 1st Sh. I. 1st Tr. E.	8 23 9 45 10 38	7 3·0
Mon	27	17 7 17 55	Occultation of B.A.C. 5023 (6) Reappearance of ditto	1st Oc. D. 3rd Ec. R. 1st Ec. R.	5 42 6 7 40 9 15 27	6 59·1
Tues	28	18 19	Occultation reappearance of 19 Scorpii (6)	1st Tr. E. 1st Sh. E. 2nd Tr. I. 2nd Sh. I. 2nd Tr. E.	5 7 6 27 7 45 10 27 10 28	6 55·2
Wed	29	16	Conjunction of Moon and Mars 0° 27′ N.			6 51·3
Thur	30	0	Conjunction of Moon and Mercury 0° 13′ N.	2nd Ec. R. 3rd Tr. I.	7 17 51 12 22	6 47·4
Fri	31	1 56 15	● New Moon Eclipse of the Sun Jupiter at quadrature with the Sun			6 43·5

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN DECEMBER, 1880.

By W. R. Birt, F.R.A.S., P.S.S.

Petavius, Vendelinus, Langrenus, the Pyrenees mountains, Guttemberg, Capella, Isidorus, Fracastorius, Beaumont, Catharina, Cyrillus, Theophilus, determine accurately the direction of the cleft in the central mountain of Theophilus. The Altai mountains. Are these indications of water having come from the plain above or south-east of them? With this object examine the neighbourhood of Sacrobosco, Gehen, Almanon, and Tacitus. Eastward of these, but west of the first lunar meridian, are the western part of Waller, Aliacenis, Werner, Delaunay, Faye, Donati, Airy, and Argelander; the above, except Airy and Argelander, are in the fourth or S.W. quadrant between 20° and 40° S. latitude, and come into sunlight before the moon attains her first quarter. In the continuation of this zone 20° to 40° south in the eastern hemisphere which comes into sunlight between first quarter and full we have the eastern half of Waller, Purbach, Thebit, Mare Nubium, Pilatus, Nisiodus, Cichus, Capuanus, Mercator, Campanus, Bullialdus, Mare Humorum, Vitello, Lee, Doppelmayer, Musinius, Cavendish, Byrgius and Eichstadt.

Corinium House, Romford Road,
Stratford, Nov. 22, 1880.

TOTAL ECLIPSE OF THE MOON,

Dec. 16, 1880, partly visible at Greenwich.

	G.M.T. h.	G.M.T. m.
First contact with the penumbra... ...	0	31·5
First contact with the shadow	1	44·5
Beginning of total phase	2	54·0
Middle of the eclipse	3	38·8
End of total phase...	4	23·6
Last contact with the shadow	5	33·1
Last contact with the penumbra	6	46·1

Magnitude of the eclipse, 1·390.
At Greenwich the moon will rise at 3h. 46m.

PARTIAL ECLIPSE OF THE SUN,

Dec. 31, 1880, visible at Greenwich.

	h.	m.	
Beginning	1	39·7	G.M.T.
Greatest phase	2	35·4	
Ending...	3	28·2	

Angle from north pole of first contact, 69° towards the west
" " " last contact, 34° towards the east
Angle from vertex of first contact, 84° towards the west
" " " last contact, 5° towards the east
} for direct image.

DUN ECHT CIRCULAR No. 11.

Last night at 11h. 10m., Dun Echt M.T., Mr. J. G. Lohse discovered a faint Comet in Lacerta. Owing to bad weather no observation was secured until 15h. 30m. 19·2s., when the place was—

		Motion in 24h. ...
α	22h. 45m. 54s.	+6m. 58s.
δ	+42° 33′·7	+68′·5

Dun Echt Observatory: LINDSAY.
1880, November 8.

DUN ECHT CIRCULAR No. 12.

From the three Dun Echt observations of the new comet—

	Dun Echt M. T.			α Comet.			δ Comet.		
	h.	m.	s.	h.	m.	s.	°	′	″
Nov. 7	15	30	19·2	22	45	53·83	+42	33	43·3
9	7	27	0·6	22	58	6·47	44	30	41·4
10	6	10	29·2	23	5	47·33	45	35	51·2

we have computed the Elements and Ephemeris :—

T = 1880 November 6·94994 Berlin M.T.
π = 40° 24′ 10″·6
☊ = 300 49 41·1 Mean Equinox 1880.
i = 7 22 13·1
log. *q* = 0·043314
Direct.

Ephemeris for Berlin Midnight :—

1880.	α		δ		log. *r*	log. Δ	Brightness.
	h.	m.	°	′			
Nov. 7	22	44·5	+42	20	0·0433	9·2915	2·46
8	22	51·7	43	30			
9	22	59·3	44	40			
10	23	7·4	45	49			
11	23	16·2	46	56	0·0443	9·2789	2·59
12	23	25·4	48	1			
13	23	35·3	49	3			
14	23	45·8	50	1			
15	23	56·9	50	56	0·0468	9·2738	2·62
16	0	8·7	51	47			
17	0	20·9	52	32			
18	0	33·6	53	11			
19	0	46·6	53	45	0·0507	9·2778	2·53
20	1	0·1	54	12			
21	1	13·7	54	32			
22	1	27·4	54	46			
23	1	41·2	54	53	0·0559	9·2910	2·32
24	1	54·8	54	54			
25	2	8·1	54	49			
26	2	21·1	54	37			
27	2	33·7	54	18	0·0624	9·3129	2·04

The computed place for Berlin midnight on October 11 is—

α = 21h. 29m. 4s. δ = +16° 47′·0

The comet is therefore identical with the one discovered by Mr. Lewis Swift, as telegraphed from America (see Circular No. 9).

Nothing seems to have been seen of the comet in Europe prior to November 7th, nor was anything further heard from America before the above computations were completed. To-day, however, a letter was received from Mr. Edward E. Barnard, of Nashville, Tennessee, dated 1880, October 30, in which that gentleman says, respecting the comet:

"I determined its position on the 21st and 22nd with ring-micrometer.
Position, Oct. 21, at 12h. 17m., G.M.T., R.A. 21h. 42¼m. Dec. +25° 1′
" " 22 " 15 27 " " 21 44 3s. " +26 2
moving in a north-*easterly* direction. It is large, diffuse, and perfectly transparent. On 24th it was visible in my 1¼-in. finder."

The following was also received to-day:

"Smithsonian telegram Swift telegraphs Lohse's comet discovered by me October 10 identical with comet third eighteen hundred sixty-nine."

A comparison of the above approximate elements with those computed by Professor Bruhns for Comet III. 1869 renders an identity in the highest degree probable, especially as the descriptions of that object accord with what is now visible.

Lord Lindsay's Observatory,
Dun Echt: 1880, November 13.

RALPH COPELAND.
J. G. LOHSE.

DUN ECHT CIRCULAR No. 13.

On November 18th, the undersigned, using Professor Pickering's method of search, discovered a small, bi-nuclear, gaseous, planetary Nebula in

$$\alpha = 21\text{h. } 2\text{m. } 11{\cdot}8\text{s.} \quad \delta = +47^\circ\ 22'{\cdot}2$$

for 1880; or in Pos. 240°, Dist. 22″ from D.M. +47° 3289.

As it is evidently an interesting object further details will be given as soon as the investigations in progress are completed.

Lord Lindsay's Observatory,
Dun Echt: 1880, November 22.

RALPH COPELAND.

W. CERASKI'S NEW VARIABLE.

From *Astronomische Nachrichten*, No. 2343.

Corrected position of W. Ceraski's new variable in Cepheus.

T Cephei, R.A. 21h. 7m. 33s. Decl. N. 67° 54′·4.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1880.
Gray, T. P.
Walter, Rev. H. S.

To Jan., 1881.
Denning, W. F.

To March, 1881.
Stothard, Dr.

To Dec., 1881.
Smith, H. S. S.

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